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THE PSYCHOLOGICAL REVIEW

LOGICAL CONSTRUCTS AND PSYCHOLOGICAL THEORY

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This article will attempt to clarify some points which arise in that branch of behavior theory sometimes known as the "theory of learning." Since this paper is mainly concentrated on methodological issues, rather than purely psychological ones, the discussion of the psychological problems will be in general terms, and in particular it will be assumed that the problem of "learning" can be reduced to the problem of "reinforcement." There exist various theories of reinforcement and those of Hull and Tolman will be regarded as representative. It is hoped to show that their differences are largely a result of different methods of theory construction. The contention is that one large part of the problem of "learning" theory can be clarified by a double analysis of existing theories, one from a semantic viewpoint and the other involving a consideration of the place of logical constructs. Also overlapping the semantic question and greatly relevant to the theory builder is the discussion between operationism and pragmatic realism.

In fact, the general problem is that of model making, which in turn leads to a comparison of "molar" and "molecular" analysis. We hope to show, indeed, that the supplementing of "molar" analysis by "molecular" analysis is necessary for the solution of the problems of reinforcement. ("Molecular" will be taken in this article in the approximate sense of Hull to mean neurological, and "molar" to mean "psychological" (12).)

THE PROBLEM OF REINFORCEMENT

Theories of reinforcement, such as an "expectancy" theory of Tolman or an "effect" theory of Hull, may be investigated both with respect to their differences and their similarities. The fact is apparent that these two theories represent generalizations from different sets of experiments which in all probability exhibit different aspects of behavior. Thus one is tempted to say that the two principles of expectancy and effect are both lacking in generality and refer to "learning" on different levels of abstraction.

There has been a tendency recently for the Hull and Tolman theories to approach an agreement (6, 7, 19, 24) which may be connected with the mixed use of operational and realistic models. There is, however, still present some measure of disagreement which is the direct result of ambiguity involved in the use of logical constructs and in the use of ordinary language.

Meehl and MacCorquodale (15) have suggested that the ultimate nonverbal differences between Hull and Tolman at this stage of development hinge on the centralist-peripheralist problem and may be made to depend on the meaning to be attached to the word "response." These differences can be traced to different historical backgrounds of the two

theories and thus to the acceptance of different philosophical directives for model building.

It is clear that theories may be inadequate for various reasons: (*a*) They may be false as a model of empirical events. (*b*) They may be too specific in view of the dimensions of their assumptions. (*c*) They may be too general and allow insufficient prediction. (*d*) Differences may arise between theories because of their use of apparently incompatible models, and in their confused dimensions and degrees of approximation. (*e*) There may be semantic confusion in the definitions and orders of abstraction, etc. (*f*) The philosophical directives used may be different for different models, although the directives used may not be explicitly stated. Among these reasons, of which one or more may apply to any theories, Hull and Tolman may seem to disagree in (*b*) and (*c*). Hull's degree of definiteness may be at variance with the dimensions of his assumptions and Tolman's relative vagueness fails to give the necessary predictability. But it is in (*d*) and especially (*e*) and (*f*) that we are interested.

Before the discussion of the disagreements, it may be worth while to say a word on the latest Hull revision (7) and its new measure of agreement. It is largely in the clear-cut recognition that performance and "learning" are not identifiable that Hull has moved toward a compromise with Tolman. The price of identification still adhered to by some theorists in essence is to facilitate theory operationally at the expense of arbitrariness. Hull now makes a clear-cut distinction between ${}_{S}H_{R}$ and ${}_{S}E_{R}$. This paves the way for the view that reinforcement affects ${}_{S}E_{R}$ and not ${}_{S}H_{R}$, and that performance is reinforced independently of "learning." The theory can then be made to allow for sudden changes in performance, which had previously been better accommodated by the Tolman constructs involving "latent learning," etc. This step of agreement is involved because of the implicit acceptance of a mixed operational-realistic model in the place of a purely operational approach. The next step may well be from the other side in an attempt to give the Tolman constructs something approaching operational definitions. The above is intended to suggest that in model building the use of operationism and pragmatic realism are not mutually exclusive, and that it is probable that operationism should be regarded as a borderline or limiting test to an otherwise realist model (1, 2, 4, 5, 8, 20, 21).

Most of the remaining differences between Hull and Tolman seem to fall on definitions and interpretations of those definitions. In particular they appear to surround the ambiguity of words such as "reinforcement," "reinforcer," "response," etc. Also we can, as Seward (19) has done, reduce the differences in one aspect to those of constructs like "habit formation" on the one hand and "mobilization of demand" on the other. The conclusion that the constructs used are ambiguous and may be interpreted in a variety of different ways seems inescapable, and the tracing back of definitions leads to different assumptions dictated by different a priori directives for theory building. These points it is now hoped to clarify in turn.

SEMANTICS, THEORY BUILDING AND THE USE OF LOGICAL CONSTRUCTS

The above brief statement of differences over the theory of reinforcement leads us to reconsider the problem of theory building in psychology, which in essence is the same as that in all other sciences. We have a theoretical model which is of course an abstraction from observations of empirical events and is couched in verbal terms

(sometimes mathematical). The first essential point is the fact that communicable theories in linguistic form involve abstracting. As Korzybski puts it (9), the "word" is not the "object," or perhaps more generally, our theoretical edifice or general model is not the empirical world. We must clearly distinguish between "designata" (nonverbal observable patterns or events), statements about designata, statements about statements, and so on. We cannot assume that the mirroring of designata on our levels of language is unique and unambiguous or that it sets up in the lowest level an isomorphism between language and nonverbal events.

It is of first importance not to identify models and empirical events nor to assume that a model is adequate to all the variables of the empirical events.

The above analysis is also independent of the question whether the lowest level of abstraction in the hierarchy be defined as observations actually carried out (operationism) or as the empirical world which is to be observed (realism). The fact remains that theory building is verbal, and there are many words that are ambiguous if considered out of specific context. These equivocal words may be defined in terms of other words, but even then we are ultimately led back (a difficult process in practice) to a set of undefined or ostensively defined terms which represent the inevitable initial assumptions of our theoretical system—assumptions that are ultimately guided by a priori philosophical directives for theory building.

The above suggests that apart from a haziness involved in the carrying out of operations due to some general principle of indeterminacy, there is also an arbitrariness and/or haziness right at the foundations of our theory. Awareness of the limitations of our model, awareness of the fact that all scientific theory involves approximation is the first step towards offsetting this basic difficulty—"consciousness of abstracting" is Korzybski's term. Korzybski's own model (9, Chaps. 25 and 26), called the structural differential, is worth close attention, but in the form given it seems to represent philosophical realism only. We have followed Korzybski's general rule of using quotation marks about words which appear to be obviously indeterminate in meaning. Thus as Marx (14) has pointed out, there is no precise meaning for words such as "perception," "learning," etc.

We are now led to a consideration of "logical constructs." We accept the differentiation of MacCorquodale and Meehl (13) with respect to intervening variables and logical constructs with the warning that there is of course no clear-cut point of distinction between one and the other. Further we follow Marx in his proposition that constructs act as platforms of a temporary nature which ford gaps in the theory and cloak observable which must ultimately replace these constructs. Now since we do not know precisely what sort of river the logical construct is bridging, there is implicit in its presence a vagueness called "surplus meaning" wherein lie both its strength and its weakness. There must be here an ambiguity which in a general way will vary as to the size of the gap to be filled. A great deal of the vagueness is overcome or minimized when the constructs are used in conjunction with operational definitions. In the sense in which Hull and Tolman use constructs this is not always possible and we shall discuss this point in the last section.

The result of our criticism of logical constructs is to place them, at least when used nonoperationally (i.e., realistically), in a category of "many-meaninged terms" and of ostensively defined

or nondefined basic terms of our verbal system. At every one of these points intervene private and personal usage and looseness of fit engendered by a priori philosophical directives: (*a*) mechanistic, (*b*) teleological, etc.

One further point is the well-known difficulty of mixed models. In physics, for example, the electron may be regarded as particle or wave, but cannot be regarded as completely one or the other. This is quite satisfactory as long as one is "conscious of abstracting" and is using a different model for different dimensions. In psychology, on the other hand, teleological models and mechanistic models, even realist and operational models, tend to be regarded as mutually exclusive. This seems quite incorrect and would appear to be the result of lack of knowledge and understanding of theory building.

A Way Out

The foregoing analysis presents something of an impasse. The claim is that our models are confused in some respects, particularly with respect to their levels of abstraction and dimensions; that there is a degree of ambiguity arising in at least three different levels due to the use of "many-meaninged terms," "logical constructs," and to the inevitable existence of "undefined terms." Much of the vagueness has been removed from such subjects as physics due to the use of the relatively precise language of mathematics and to the consciousness of dimensions. The high degree of vagueness could be diminished in psychology, even in terms of our verbal models, if we were able to state more precisely the existing variables and also increase the number of observables. Thus the continual replacement of logical constructs is necessary; and while many may be transformed into operationally valid intervening variables, the ability to do this, which is involved anyway in the ultimate replacement of constructs, must depend on the increase in the number of observables. Further, since the bulk of constructs in learning theory appear to refer to "organic states," this appears to constitute the clearest evidence for increased concentration on "molecular" levels of analysis.

In the above context, the constructs such as "cognitive maps," ${}_{S}E_{R}$, ${}_{S}H_{R}$, are obvious examples. They are necessitated by the distinction between "performance" and "learning." The process of reduction to intervening variables by operational definition or to complete replacement in both cases implies an increase in the number of variables, and thus in the degree of observability.

Apart from this immediate aspect, it is clear that "molar" analysis by itself is quite insufficient for the adequate analysis of the bulk of behavior problems. It is true that it is a vital part, as "molecular" analysis alone may be inadequate. But it seems certain that there must also remain in "molar" analysis alone a degree of approximation, uncertainty, etc., altogether too crude to answer many of the vital questions demanded of behavior theory.

"Molar" analysis on its own seems to contribute to the vagueness inherent in the use of logical constructs. It is too much like trying to use 6-ft. planks to bridge 24-ft. gaps. The cure for this, in part, must, of course, depend on technological advances such as improved instruments of observation, and also the extension of psychological experiments to "molecular" analysis on a larger scale.

The above propositions suggest that the answer to Postman's three questions (**16**):

(1) What is the agent responsible for reinforcement?
(2) What is it that is reinforced?
(3) What is the basic mechanism of reinforcement?

may begin with question (3) rather than (1) and (2) as is suggested by Seward's recent analysis (19). Thus Wolpe's (27, 28) approach, although perhaps oversimplified, has much to recommend it.

"Consciousness of abstraction," appreciation of dimensions and the nature of approximations and degrees of errors, coupled with a sound knowledge of system building, would seem to constitute the greatest safeguard in the effort to overcome the semantic problems. These will also be simplified, however, with the integration of further levels of abstraction and isolation of variables, which will perhaps allow the use of mathematical language for a great many of the basic problems of behavior theory. The realization that there are points for the inclusion of personal tastes in present theory building may go some way to stop arguments between groups whose views are complementary and capable of synthesis, rather than mutually exclusive.

The next step would seem to involve considerably greater attention to general system-theory; for clearly the theory as a whole, at all levels of science, will affect the parts as well as the whole. The point is illustrated here by the claim of Von Bertalanffy (25, 26) that "teleology" is a formulation derived from a closed thermodynamical system which when used in an open system becomes devoid of meaning. A somewhat similar problem over the word "purpose" arose in the discussion between Taylor (22, 23) and Rosenbleuth and Wiener (18). This last discussion illustrates well the one particular form of semantic confusion that may, if unclarified, vitiate scientific theory. The clarifying points made by Rosenbleuth and Wiener are particularly instructive, especially the fact that degree of causality implies a form of analysis, i.e., statistics and probability theory, and that the use of a term like "purpose" arises as an artifact of abstraction and depends on the characteristics of the model as a whole.

Krech's recent analysis (10, 11) seems particularly fruitful in view of what has been said. The construct "dynamic system" is rooted in neurology and is stated in a form favoring the necessary reducibility—ultimately, of course, to endocrinology, colloidal chemistry, etc.

Two further points may be mentioned. Pratt's analysis of psychological theory (17, 20) points out that all science has the same subject matter. This view is implicit in the foregoing analysis and the truth of Pratt's contention is a starting point for an extension of all scientific theory to the level of general system theory on the one hand, and to the differentiating detail of type of model and dimensions of our particular areas on the other. The second awkward point of testability (3) or verifiability, etc. has been omitted from this note; the belief here is that the awkwardness is largely removed by semantic analysis; otherwise it goes beyond the scope of this paper.

All that has been said in this paper is in essence programmatic. It is hoped to build more positively and specifically upon this analysis in a later paper. It seems to the writer that a program of "integration of science" is more urgent at this time than a greater specificity with respect to particular models.

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PHYSICAL PSYCHOLOGY

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Predominant Traits of Systematic Psychology

The dominant feature in psychological theories during the last two decades seems to have been a concern for logical precision, with the object of fashioning a structure like physics. As physics has its logical constructs, so psychology has developed a great variety of hypothetical concepts and intervening variables, generated in this case by the psychologists' efforts to organize behavior or "experiential" data, and having no other necessary reference. In some cases even the language describing the data is of a sort peculiar to psychology. Much research has been guided by this kind of thinking, though not all of it. Some deals only with physically defined variables. It is the purpose of this paper to explore the rationale for this latter kind of work and see whether it might be generalized. To designate this sort of thinking conveniently the term "physical psychology" is suggested.[1]

This situation in psychological theory seems to have come about because of the severe criticism during the 1920's of the uninhibited "neurologizing" which was then current. In an excess of caution, many writers undertook to construct a pure science of behavior as though no other knowledge existed. So we have terms like drive (Hull, 5), psychic force (Lewin, 8), cognitive maps (Tolman, 16), conflict (Miller, 10), etc., terms which refer only to the class of data they were constructed to explain. Somewhere in the domain of science, no doubt, there must be a choice of nonreducible terms, but it is not necessary that each particular science should make a new choice. The semantic clarity and sophistication of the enterprise are, of course, a great merit of the recent psychological writing. But the application of scientific methodology to the subject *de novo*, as it were, is a procedure which has perhaps already led us as far as it can. Instead of seeking the ideal of a science *like* physics, we might do well to try making psychology a *part of* physics, or more precisely, a part of the body of general science which regards reducibility as an obvious requirement of a good theory.

With as much elbow room as nonreducibility allows it is not surprising that theorists can execute a wide variety of maneuvers. But there arises a suspicion, at least, that there is no unique solution when the theorist is allowed such an open field. Paradoxically theories are too easy to make. If a theorist, having taken a few peeks at the data, selects a set of hypothetical constructs and uses them in his equation (mathematical or verbal) which predicts the data, he is restricting himself somewhat, but probably not enough to exclude alternatives. Another set of equations with different terms might accord with the same facts, or facts that are so nearly the same as to be nondiscriminable. If this suspicion is correct, there will be no "crucial" experiments within the sphere of behavior to decide between a good number of theories.[2] Each

[1] Logical empiricists have, of course, recommended "physicalism" for some time, but they have not gone so far as the position here presented. Probably they would not insist upon the kind of reduction here advocated. Therefore, it seems advisable to distinguish between a physical and a physicalist psychology.

[2] Meehl and MacCorquodale (9) find this is true of the Hull and Tolman theories.

one will be able to assimilate any possible facts.

For one who is committed to an independent behavioral science there remains only an appeal to parsimony or elegance. Although theories are usually believed by their advocates to have these qualities, an explicit demonstration of them is uncommon. The principle of simplicity is in fact not very simple to apply. It is not very clear whether one should try to minimize constructs, assumptions, propositions, or all of these. In any case, to apply the principle of parsimony, a definite field of observations needs to be marked out; for if one is to compare two explanations, it must be certain that they cover the same territory. In general, one theory may be expected to operate more conveniently in one part of a field, another in a different part. To evaluate them one needs to decide which part is more important and how much awkwardness here is equal to how much awkwardness there. It is no wonder that psychologists shrink from such a comparison. Parsimony is doubtless needed as a last resort in the philosophy of science, but it does not offer an attractive short cut.

THE POSSIBILITY OF PHYSICALLY RESTRICTED THEORY

Conceivably, a unification of theory could be achieved by fiat if writers would agree to one particular set of logical fictions. At periods in the history of psychology, something of the sort seems to have happened; periods which have been the dullest and least profitable of all. For scientific progress, it seems one must be able to consider alternatives and have some criterion for deciding between them.

The criterion here proposed is the observability of the thing hypothesized instead of merely the observability of the behavioral consequences. Only those constructs would be tolerated which could be reduced to those of physical science. To illustrate: "Hunger drive" defined as an influence on behavior generated by lack of food is a construct which is not restricted by such a rule (Spence, 14). On the other hand, a blood sugar deficiency as a possible connecting link between food deprivation and certain behavior is a construct of the second kind. (Although constructs would be reducible to the ultimate ones of physics, they need not, of course, be so reduced on all occasions.)

Such a rule narrows the area in which a theorist may grope, and in so doing, confers a number of benefits. With it psychology can form junctions with the underlying sciences. In modern times, chemistry, geology, biology, and physiology, for example, have for the most part restricted themselves to explanatory constructs of the "reducible" sort. They have, as it were, extended the domain of physics into their fields; or to put it better, they have had a good deal of success constructing one general science with the same language throughout. In an earlier time they did not always proceed so, but now constructs like entelechy, valence, and central excitatory state have vanished or acquired a "physical" meaning. On the other hand, in the social sciences, the use of "free" constructs seems to be rather prevalent. In these, as one would expect, there is not much connection between one science and another, nor even sometimes between one topic and another in the same science.[3] So, if psy-

[3] To be sure, there have been complaints that the biological sciences, for instance, are wanting in general unifying principle, and, therefore, fail to be truly scientific. A "system of physiology" would, indeed, now fall upon the ear like an echo from antiquity. Yet blundering about as they may be in comparison to mathematics or physics, these empirical sciences nevertheless have their triumphs and are hardly to be despised as models of con-

chological theorists can work under the restraining rule, there is a chance that the field of general science will spread into the realm of psychology, while lines of communication with the older provinces are kept open.

One way to reach this Utopia of unified science would be to start with a likely set of "behavioral" constructs, by hypothesis give them an independent meaning, and proceed to experimental test. A behavioral theorist would postulate a variable which has such and such a relation to behavior, to the stimulus, or to both, and thereby set the problem for the physiological experimeter of discovering, somewhere in the organism presumably, an actual condition or event which has the stated relations. An enterprise of that sort has often been favored; it seems to be the hope of some "behavioral" theorists (Hull), and even physiological psychologists (11), (3) appear content to take their framework from behavioral theory and try to fill it with physiological referents. Krech (7) complains that the theorist in such a proceeding gives the experimenter no clue about where to look. He may also send the experimenter on a fool's errand. For the postulated construct may belong to a set which is only one of a great number of alternatives, only one of which could have a physiological meaning. By good fortune the experimenter might hit upon the right formulation. But to proceed so is to give up the guidance extra-behavioral knowledge may offer in the original choice of concepts: a guidance which we badly need if the preceding remarks are correct. To put that knowledge out of mind in psychological theorizing while hoping for later independent verification seems rather like a game of darts played blindfold. Later inspection may show the aim was good, but why play that way anyhow?

Instead, the originator of explanatory concepts can be guided by the rule that they should have an operational meaning outside the realm of behavior. The theorist, in other words, would construct a hypothesis with a much broader set of facts in mind; indeed all the facts that are represented by the "physical" constructs he would use as the behavioral events he is theorizing about. Of necessity his freedom is reduced, as the foregoing argument would have it. Another of the effects would be to place physiological and behavioral psychology on the same footing: physiological psychologists would no longer strain their wits to find a physical lineage for constructs of pure Olympian birth. Perception, emotion, motivation, habit strength, expectancy, and other terms which offer names for ignorance (or maybe a trifle more) would probably disappear.

Other criticisms of these concepts could be made: It may be difficult to make such terms very definite by any means at all. But, since they are not meaningful in a physical sense, they would fail to meet this canon for explanatory concepts, however well specified in some other way.

Terms which would fit the proposed restriction would surely be physiological in good part, but not altogether so. Physiological terms would be needed in place of properties of the individual (permanent and temporary) that are mentioned or implied in so many theories, and in place of hypothetical processes that take place between stimulation and response. Theories might refer, for example, to oxygen level or hormone content of the blood, neural conduction characteristics, or state of contraction of such and such muscles, as intervening between a certain stimu-

duct. If they have a unifying principle, it is the self-denying principle of using only general scientific concepts.

lus and its response, or determining one or another kind of response to a stimulus. (There is no reason, of course, why theories should be restricted to the nervous system.)

But the psychologist also needs to describe external factors that influence behavior. His task as a whole would be simply to uncover the chain of events which leads to a given action. The immediate precursors will be within the organism, of course, but the chain will certainly extend to events in the world outside. It is likely, further, that the chain will be found to cross the border of the skin repeatedly if it is traced far enough. These external events may be called stimuli for convenience (though the name seems rather misleading), but in any case there would be no reason for demarking them very sharply from the internal events. Both would be treated the same way. The physical psychologist would need to make hypotheses about these external events as well as about those inside, and here the appropriate concepts would be either immediately physical or reducible to physical through the inorganic sciences.

The outside world seems such a solid thing that some theorists feel that they can lay all their burdens on it. But when overlaid with psychological portmanteaus like valences, psychic distances, stimulus functions, and signgestalts, the solid terrain seems to vanish beneath the waves and leave the baggage floating. These abstractions are derived, evidently, from observations on behavior in the same manner as those which seem to refer to the organism, and they suffer from the same indeterminacy. For a "stimulus" (external event) to qualify under the proposed canon, it would have to be something which an experimenter could ascertain without there being any organism for it to work on.

Behavior Description under a Reductive Rule

A physical description of the *data* that are the starting point of psychological theory would evidently lay the groundwork better for a physically bound *theory*. If the latter is a proper goal, as has been argued, the first step should be in that direction too. But unfortunately a great deal of psychological data is not given in physical terms, even when it might be; and some theorists expressly defend the use of nonrestricted terms. Cantril (2) proposes that any sort of descriptive terminology the psychologist can think of will be a good basis for beginning an investigation. If nonphysical terms are to be excluded, two questions need answering: (*a*) what sort of description would result from the application of the physical criterion? and (*b*) what, if anything, would be lost by the abandonment of the nonphysical descriptive terms?

In a sense the physical specification of data is unavoidable. The events that constitute the subject matter of the science, it is assumed, are the actions of individuals; that is, their movements and their utterances, and each single event has its physical characteristics whether noted or not. Even though a psychologist should say he is describing subjective experiences, he is using the utterances and sometimes the actions of his observers as indicators, and these have their physical characteristics. Such characteristics are available then in all data, but they are used in different ways in the next stage of the scientific process.

Because no events apparently are the exact replicas of others, the investigator must form them into groups on the basis of certain resemblances before he can continue. It is in this classification that psychologists differ in their usage and there seem to be three principal

bases on which a classification is carried out.

1. The classification may be explicitly based on some physical characteristic of the activity which is the response event. The plan would be, then, to study the class of events marked by a forearm flexion, for example, or the exertion of a given force, or the occurrence of a certain pattern of muscular or neural excitation. The consequences of the events would be disregarded in the course of inquiry into the origins. The physical nature of the definition is obvious in this case. It seems to be the kind of procedure that Guthrie (4) has recommended.

2. No less dependent on physical specification is a plan of classification which would group together those events which have the same effect on objects around the individual or on the object-individual relationship. On this basis all actions resulting in, say, bar pressing, or the transportation of goods to a given point, or the approximation of an individual and an object would be treated as unit classes for the purpose of investigating their determiners. This kind of classification has been favored by Skinner (13) and Nissen (12). As they point out, the specifications here are physical also, although they refer to effects outside the organism.

3. A third manner of classification depends on the experimenter's response to the subject's behavior, a response made without explicit rules. The subject makes a response and the experimenter (or another observer) judges that the response is, for example, aggressive, or submissive, or insightful, or possesses any other adverbial quality that happens to be of interest to the experimenter. The experimenter, that is to say, makes a verbal response to the subject's behavior and then considers all behavior which provokes the same verbal response from the experimenter as belonging to the same class. It is often thought that in doing so, he is defining classes which have no common physical characteristics. But the common physical event is present nevertheless; it is the response of the observer, and behavioral events which have similar consequences in the observer's behavior are assimilated to the same class.

Though all three methods are dependent upon a physical characteristic, there is a question whether they all depend upon a physical characteristic of the *subject's response*. About the first there is no question since it is expressly based upon such characteristics. The second is a little further removed. Whether it would be a strategic beginning very likely depends on whether the state of external affairs has a feedback effect so that it continues to act as a stimulus until a particular end result is reached. To what extent it does can be found out, of course, only by experimentation, for there seem to be examples of responses so controlled and not so controlled, and it would be very hazardous to take for granted the universality of either kind of response mechanism. It would be a handicap, furthermore, to have the response specified in terms of external results when they play no role in the regulation of the response.

The third type of response specification starts a long way from the response event. A good many factors other than the subject's response are likely to condition the observer's response. This is partly determined by what he is observing, but also no doubt a good deal by his vocabulary, his predilections, his standards, etc. All such things extraneous to subject's behavior would act as "noise" to corrupt the information which is desired. Possibly there are instances, nevertheless, where such reports of observers need to be used as a starting point. They may be justified on the same ground as the use

of the external consequence according to the second type of approach. That is to say, if the subject persists in a line of action until a certain response is produced in the observer, it would be appropriate to class together subject's responses which lead to the same result in the observer's response. A physical account would begin with the observer's final action and describe in physical terms what can be seen of the actions of both subject and observer leading up to it. In sum, there will be a system with loose couplings in both directions and extra systemic influences entering at both points. Experimental or other analytic procedure would be necessary to uncover what factors in the behavior of the two and what outside factors play the role of stimuli for the two.

No barrier therefore stands in the way of a physical description of all sorts of behavior. The physical aspects are there if we but note them. Nor does it seem that anything would be lost by such tactics except some unprofitable entities named by the many adverbs in the common language.

In the course of statistical treatments of data which are physically defined in the beginning, psychologists may be inclined to forget this humble origin and imagine that some statistical abstractions represent causal factors underlying the observations. "Measured" traits, whether simply test scores or refined to Thurstone factors, sometimes seem to have this specious reality, and to present the theorist with the illusory problem of how to connect such nonphysically defined entities with the realm of general science. The original data in these cases are, of course, the responses people make in taking the tests—responses of putting pencil marks in certain blanks rather than in certain others. These are physically defined data of the second class, which may perhaps be well summarized by the statistical abstractions, but hardly originated or explained by them.

In very much the same case are the dimensional variables that come out of psychophysical research. The sensation scales of various kinds, loudness, brilliance, volume, for example, are based on concepts which are not a part of the common scientific language. It is not possible to reduce any of them to physical units except by means of behavioral data. As Bergmann and Spence (1) observe, they are logical abstractions from behavior data, at bottom just summary descriptions of certain responses. However useful these may be for some purposes, it is still the responses which need to be accounted for, not the "as-if" world which would account for the responses if it had a real existence. If one succumbs to the temptation of reifying terms like threshold, constant error, sensation dimension, he is attributing the physical responses to nonphysical variables.

In hoping for more valid measures of such things, Stevens (15), for example, implies that they exist independent of the responses (yes-no utterances in a discrimination experiment). This is a proposition which surely ought not to be taken for granted. A hunt for the physical parallels of these entities (the "physical dimensions of consciousness") would very likely have the same outcome as a polar expedition to discover St. Nicholas.

THE GENERAL EFFECTS OF A PHYSICAL PROGRAM

A psychology written entirely in the common scientific language would differ a good deal from general psychology as it is now presented and eliminate much that is now discussed as psychological theory. At the outset the burden of distinguishing psychology from physiology on the basis of source or kind of

data, scale of data, a method or purpose of study would be lifted.

Another burden which psychologists would no longer be obliged to sustain would be the definition of the present main categories of psychology. These chapter headings, such as perception, motivation, emotion, personality, etc., seem troublesome to most writers already, and here and there some of them have been discarded, e.g., by Keller and Schoenfeld (6). No doubt these words were originally intended as names for mental entities, but writers at present would try to avoid such a naive reference. Consequently, at the beginning of each topic a writer or lecturer will say, in effect, "Now I shall speak about ————. But, of course, you are to understand that there really is no such thing as ————." The terms, of course, are not reducible to physical components and have to be treated as logical constructs or self-defining descriptions.

What categories would replace these ancient ones as a basis for organization, it would require a prophet to say. But, in any case the content would be statements about the connections between (*a*) physical conditions in the environment, (*b*) the physiological, physical, and chemical conditions and events in the organism, and (*c*) the physical consequences. It is impossible to answer the question "What category and concept would replace such and such a one now in use?" It is not likely that any new concept would exactly replace an old one; indeed, in such a case, there would be little gain. One may hazard a guess in a particular instance to serve as an illustration. With the abandonment of "motivation" as a category, the construct of "motive" or "drive" defined in the current roundabout fashion would also disappear. Some of the experimental results now handled with these terms could possibly be assembled into a topic "persistent systemic physical and chemical states affecting responses." This would include an account of the effects of nutritional states, hormonal conditions, temperature states, etc., presenting material now included in the discussion of motivation. It would also contain material on the effects of oxygen deficiency, fatigue products, bacterial toxins, etc., which are not generally put into chapters on motivation (or in any other chapters for that matter).

In this exposition, certain material now discussed under the topic of motivation would be out of place. It would hardly include experiments which are concerned with the effect of electric shock on learning, for example. Nor would there be any reason to speak of "conflict" since physical and chemical states do not war with one another. "Ego maintenance" is obviously not in the physical universe of discourse. The facts now assembled under these subheads would be put under some other general topics.

Thus, it will be seen, the insistence upon physical science would probably result in a different organization of present material. If the argument here offered is correct, it would, when facts are available, come nearer to producing a definite psychology. Of course, the result for a long time to come would be much less tidy than the systems now in vogue. For with restricted constructions no single insight, however brilliant, is likely to reveal the theory of everything, nor a few simple postulates to imply all the complexities of life. Although there is nothing to prevent speculation from outrunning facts by a great distance, performances of this sort could not but seem rather eccentric. The sprint might too easily be in the wrong direction.

The use of physical concepts in psychology brings to mind earlier proposals such as those offered by the behaviorists

several decades ago. But their reasoning seems to have been different, their position apt to be sustained by little more than an appeal to tough-mindedness. In practice there was too frequently a simple "physicalizing" of old concepts. The terms "instinct" and "emotion," for example, were merely asserted to refer to bodily states. The traditional, vaguely defined categories and processes were taken as the starting point and a likely-seeming physiological process was proposed as their substance. This physiologizing of old psychology is, of course, a different undertaking from the construction of a physically reducible psychology from the beginning. In other words, early behaviorists were not sufficiently radical.

General psychology written according to the physical prescription is something yet to appear. Experiments in these terms are nevertheless not wanting. Many experimental reports seem to have little connection with any current psychological structure, but their implicit object seems to be the kind of behavior analysis here projected. Further, physical information can often be extracted from its bed in alien theory. Indeed, if the foregoing analysis is correct, the experimenter will actually have identified the data by physical specifications and he will often state them even when he considers them unimportant. The formulation of physical theory for the fields of psychology thus seems possible. Naturally the most persuasive evidence would be the systematic application of it even to a small area where it had not been used before. It seems as though it would work. The argument for the path offered here is basically one of expediency; it is a route which promises more in the long run. And expediency, of course, is relative. Where there is yet no map referring to the Greenwich meridian (or even to any coordinates), stories of the region may not be quite useless—so long as they do not merely instruct the traveler to follow his nose. But the explorer will want to replace them as soon as possible with readings of compass and transit.

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A GRADIENT THEORY OF MULTIPLE-CHOICE LEARNING

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Ever since Yerkes (14) modified Hamilton's (2) apparatus for studying learning behavior, the multiple-choice method has provided psychologists with a rich profusion of facts and a theoretical problem. It would be hard to point to an area outside the field of sensory psychology that matches this one for plethora of data and paucity of theory able to account for any considerable share of the data. Yerkes' original view was that multiple-choice learning requires ideation. Hunter (4) dissents from this view. Washburn (12) holds that only the middle-door problem, in which the middle one of, say, three doors is the correct choice, requires ideation, and that the end-door problem involves only a simple position habit. Burtt (1) also takes this stand with regard to the end-door problem, and Spence (9) believes that "strong support may be found for the view."

What is certainly the case is that the middle-door problem is harder than the end-door problem. It is, in fact, harder than the alternate end-door problem and the second-door-from-end problem. To quote Spence's review (9) of experimental results:

> Only one subject, a European siskin [1] (7), has been reported able to solve the middle-door problem. Four chimpanzees (15) failed in this problem, as did one monkey (13) and two pigs (16). The problem next in difficulty, the alternate-end-door, was successfully solved by two pigs (16), one of two monkeys (13), and only one of four chimpanzees. Likewise, only one chimpanzee of four was able to succeed with the second-door-from-end problem (13). This latter was also solved by three monkeys (13 and 10), two pigs, a skunk (5) and a marten (5). It proved too difficult for the orangutan (13) and two white rats (1). All subjects tested, chimpanzee, orang, monkey, pig, cat, white rat, and crow, have been reported as succeeding with the simple end-box problem.

[1] Defined by *Webster's Unabridged* as: "a small, sharp-billed, chiefly greenish and yellowish finch (*Spinus spinus*) of temperate Europe and Asia, allied to the goldfinch."

The question remains: Why is the middle-door problem more difficult than the end-door problem? It may be subjectively satisfying to laymen and psychologists alike to say that in the middle-door problem the animal is confused by the doors on either side in a way that it is not confused in the end-door problem, but this skirts the issue. Why is it more confused? It seems similarly inadequate to say that the middle door is "imbedded" to a greater extent than the end door. Indeed, such hypotheses as these are hardly more than photographs of the problem. Underwood (11), though he has no well-developed theory, regards multiple-choice learning as a species of concept formation, but this, of course, does not explain why the middle-door concept is harder to form than the end-door concept. It may be of some help to say, with Washburn and Burtt, that the one involves ideation and the other requires only a simple position habit, but whatever virtues such a dichotomous theory may have, it is certainly less parsimonious than a theory that explains the results on a single basis. Moreover, the theory of Washburn and Burtt runs counter to the widespread suspicion among psychologists that chimpanzees are brighter than siskins.

Another fact about multiple-choice learning is one mentioned by Under-

wood (11) and also by Munn (6), which is that human subjects find it very difficult to solve a multiple-choice problem when the correct choice alternates about the middle; that is, for example, when the correct choice in a five-choice apparatus is No. 2 on the first trial, No. 4 on the second trial, No. 2 again on the third trial, etc. As far as the present writer knows, no theory has yet been advanced to account for this.

A third phenomenon that needs explaining is the way in which chimpanzees shift from the correct response on one problem to the correct response on a following problem. Spence (9) noted that the animals made the change gradually. If the former correct response had been to the second door from the left end of a six-door setting, and the correct door in the new problem is the second door from the right-end door of seven doors, the chimpanzees, after failing to get reinforcement at the formerly correct door, shift from left to right, trying each door in succession.

A fourth phenomenon of multiple-choice learning, and one also remarked on by Spence (9), is the rather curious fact that in shifting from one correct response to another, all the chimpanzees, without exception, exhibited a tendency to overshoot the mark. For example, in the hypothetical case just mentioned, instead of stopping at door No. 6, the correct door, the animal, after being rewarded at that door, proceeds to door No. 7.

The hypothesis advanced here attempts to account for all four of these phenomena: (*a*) the fact that animals find the middle-door problem harder than the end-door problem, (*b*) the fact that human subjects find it very difficult when the correct choice alternates about the middle, (*c*) the fact that in chimpanzees the shift from a formerly correct response to a new correct response is a gradual approach process, and (*d*) the fact that in so shifting, chimpanzees have a tendency to overshoot the mark and respond to the next door just after being rewarded at the correct door.

Explanations of other phenomena of multiple-choice learning can probably also be deduced from the theory, and one or two of these will be suggested. But the first task is to expound the theory and show that the four previously mentioned phenomena are predictable on the basis of the theory.

The present hypothesis owes an enormous debt on the theoretical side to Spence's account (8) of the transposition experiment and a scarcely smaller debt on the experimental side to Spence's monograph (9) on multiple-choice learning in chimpanzees. Like its parent theory, this one is based on the notion of stimulus generalization, and the two theories have four assumptions in common. They assume that there is an excitatory gradient about a stimulus to which a reinforced response has been made. The second assumption is that an inhibitory gradient is set up about a stimulus when responses to that stimulus are *not* reinforced. The third assumption is that the excitatory gradient has a higher maximum than the inhibitory gradient and also has a wider extent. The fourth assumption is that a response tendency is the algebraic sum of the excitatory and inhibitory potentials.

Given the basic assumptions, let us now hypothesize some numerical values for the gradients. These numerical values may be incorrect, but that does not invalidate the theory. In order to do that they would have to be so grossly wrong as not to constitute gradients at all. Several different sets of numbers no doubt will yield adequate predictions. The only virtue of these particular sets is that they, too, will explain the phenomena. Let us assume that

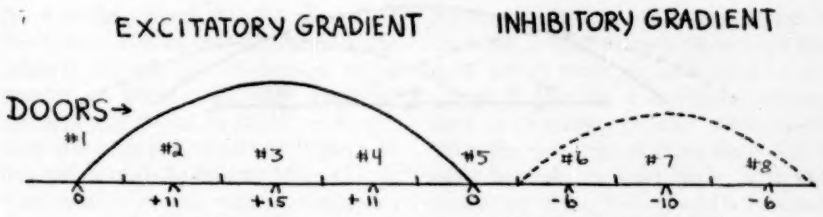


FIG. 1. Numerical values for excitatory and inhibitory gradients

the excitatory gradient has a maximum of 15 over the correct door; at one remove it has a value of 11; and it falls to zero by the time it reaches the door at two removes from the correct choice. The inhibitory gradient is assumed to have a maximum of 10, to fall to 6 at one remove and to zero at two removes. Thus the two gradients would be as pictured in Fig. 1. In computing response tendencies the excitatory gradient is given positive values and the inhibitory gradient negative ones. These values are indicated below the numbered doors. It may be noted that though the excitatory gradient is wider than the inhibitory one, it is not wide enough to have a numerical value at two removes greater than zero.

On the basis of these hypothesized values, the resultant response tendencies for the end-door problem in which door No. 3 is the correct choice would be as depicted in Fig. 2. Thus the correct door enjoys an absolute advantage over its closest competitor (door No. 2) of the difference between -5 and $+9$, or 14 points. The greater the difference between competing response tendencies, of course, the easier the learning.

This difference contrasts sharply with the situation that obtains in the much more difficult middle-door problem as diagrammed in Fig. 3. Here, owing to the overlapping of inhibitory gradients over the correct (middle) choice, the middle door has only the very slight advantage of two points over its competitors.[2] Thus the first phenomenon is accounted for by the theory.

[2] If one is interested in the question of phylogenetic differences in multiple-choice learning, it is just possible that one of the answers may lie in this: that higher organisms are capable of responding to smaller dif-

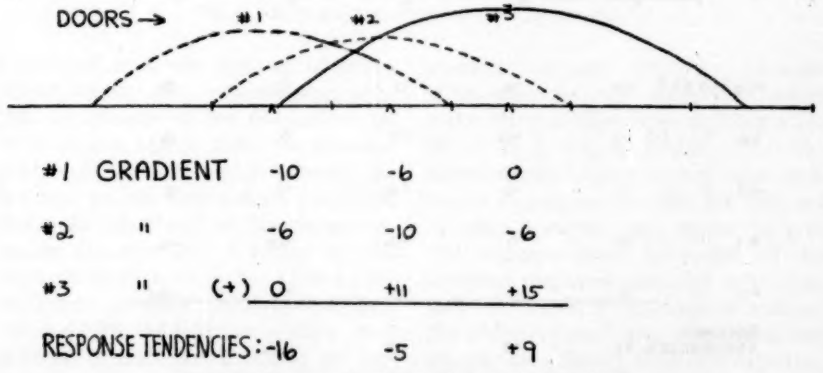


FIG. 2. Response tendencies for end-door problem

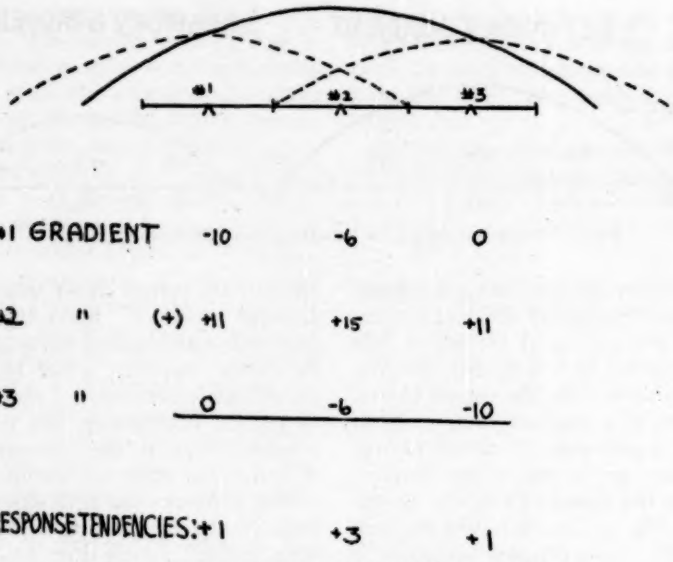


FIG. 3. Response tendencies for middle-door problem

To move on to the second phenomenon to be explained—namely, why human subjects find it so difficult to solve a multiple-choice problem when the correct response alternates from side to side about the middle on successive trials—let us suppose that we have a five-choice apparatus in which the correct choice is No. 2 on the first trial,

ferentials than are lower forms. The rather puzzling case of the single European siskin will be discussed later.

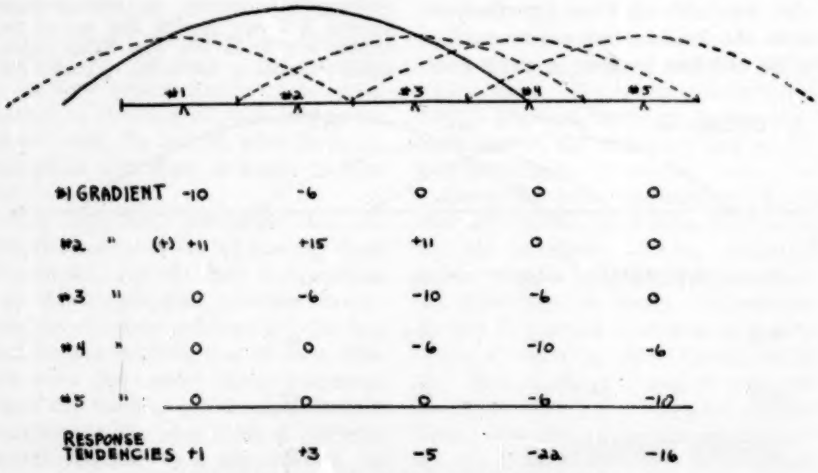
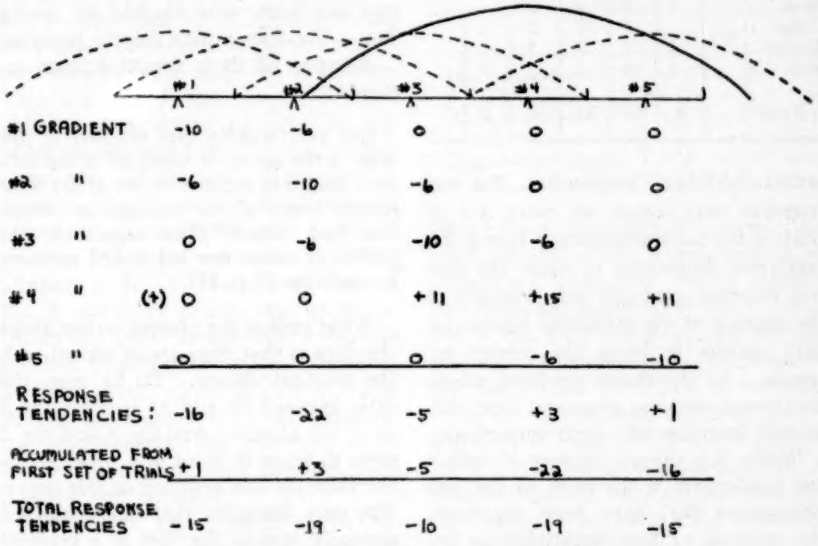


FIG. 4. Response tendencies after first set of trials

No 4 on the second, No. 2 again on the third, etc. Let us also assume that the subjects try all the choices an equal number of times. We shall thus call the first trial "a set of trials" and compute the response tendencies after the first set of trials as follows (Fig. 4). It is apparent from the response tendencies that the least likely response on the second set of trials will be door No. 4, which is precisely the correct choice. Certain response tendencies have accumulated from the first set of trials. If we compute the response tendencies that accumulate during the second set of trials and add to them the response tendencies that accumulated during the first set, we can then make a prediction about the likelihood of the correct response (to door No. 2 again) on the third set of trials (Fig. 5). Just as the inhibitory potential piled up on door No. 4 during the first set of trials, so it piles up on door No. 2 during the second set with the result that the total response tendencies make a response to door No. 2 (the correct choice) on the third set of trials as unlikely as a response to door No. 4 and more unlikely than a response to any other door. After the third set of trials door No. 4 again becomes the least likely; after the fourth set door No. 2 again becomes equally unlikely with No. 4; and so on ad infinitum. In fact, if it were not for the presence of other factors in the situation, such as the fading of the gradients through forgetting between trials, etc., the only thing that would make this multiple-choice problem soluble at all is the circumstance that the experimental subjects do *not* make each choice equally often. On the first set of trials a subject may make the correct response before he makes all the incorrect responses, and this will eliminate for this set of trials one or more of the inhibitory gradients. On the second set all the different incorrect responses may not occur, thus further reducing


#1
#2
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#1 GRADIENT -10 -6 0 0 0
#2 " -6 -10 -6 0 0
#3 " 0 -6 -10 -6 0
#4 " (+) 0 0 +11 +15 +11
#5 " 0 0 0 -6 -10
RESPONSE TENDENCIES: -16 -22 -5 +3 +1
ACCUMULATED FROM FIRST SET OF TRIALS +1 +3 -5 -22 -16
TOTAL RESPONSE TENDENCIES -15 -19 -10 -19 -15

FIG. 5. Response tendencies on third set of trials

TABLE 1

DISTRIBUTION OF CHIMPANZEES' RESPONSES IN A SEVEN-CHOICE APPARATUS
[from Spence (9)]

Subjects	Boxes						
	1	2	3	4	5	6	7
Bentia			1	14	5		
Soda			5	13	2		
Mamo			4	12	4		
Wendy			11	7	2		
Nira			5	9	6		
Al			5	6	8	1	
Josie		2	7	8	3		
Cuba			9	10	1		
Mona		2	7	8	3		
Pati		4	6	6	3	1	
Total		8	60	93	37	2	

certain inhibitory tendencies. But one response that occurs on every set of trials is the correct response. Hence the excitatory tendencies to make the correct response gradually gain strength at the expense of the inhibitory tendencies that operate to block the correct response. As the theory predicts, where the correct response alternates from side to side, learning will occur very slowly.

While this theory permits of rather pat predictions in the cases of the two phenomena that have been examined, the question of how Sadovinkova's siskin (11) was able to solve a problem that baffles many chimpanzees is one that cannot be answered conclusively without knowing exactly what responses the bird made and in what order it made them. In the light of the foregoing discussion it might be suggested that an answer may be found by applying a gradient theory to the raw data for the individual subject. By putting together a gradient theory and the fact that a subject does not always make all the incorrect responses before making the correct one, it may be possible to explain quite easily all those puzzling phylogenetic reversals such as this one, in which a bird can solve a problem that many a chimpanzee can not.

Spence (9), however, was able to get chimpanzees to solve the middle-door problem. They did not, of course, go to the middle door 100 per cent of the time; sometimes they went to other doors. But the sample of the data on their responses that Spence gives seems to lend support to a gradient theory. It is reproduced in Table 1. Spence used a seven-choice apparatus in which the two end doors were blocked off, giving him a five-door setting for the problem.

Apropos of these results Spence remarks:

> One very striking fact revealed in this table is the extent to which all of the subjects tended to respond to one of the three middle boxes of the settings, as though they had "aimed" their response at the middle or center box but lacked precision in execution (9, p. 24).

What strikes the present writer about the data is that they are in accord with the gradient theory. To be sure, the ratio between 93 and 37 is not quite 3 to 1, but almost. And the 8 and the 2 seem to argue in favor of a wider gradient than the one assumed in this paper. The only deviation that might militate seriously against the idea of a gradient is the lack of symmetry in the curve. The animals went to No. 3 60 times, as against 37 times for No. 5. But this tendency to reach to the left may be due to nothing more than their being repeatedly hand-fed by right-handed psychologists.

The third phenomenon upon which this gradient hypothesis may shed some light is the manner in which Spence's chimpanzees changed from one response to another. After being taught to respond to the second from the left end of six doors, the animals were switched to a problem in which the correct door was

the right-end door of seven doors, and it was noted that the animals shifted over gradually, trying each door in succession from left to right, although occasionally an animal would skip a door. Now, while the theory has no ready explanation for the skipping, it would seem in this process of gradual shifting that the animals were moving down a gradient, building up inhibition with each successive nonreinforcement and thus never having a tendency to back up again. Let us say that the animal keeps responding to No. 2, which had been the correct door, until the inhibitory potential outweighs the excitatory. Then the animal shifts to No. 3 which still has some excitatory potential left which generalized from No. 2 when No. 2 was the correct door. Nonreinforcement at No. 3 piles up inhibition not only at No. 3 itself but also at No. 2 and No. 4 in equal quantities. But inhibition at No. 2 was greater than at No. 3, and much greater than at No. 4, before the shift, so now No. 4 has the greatest tendency to evoke a response and the animal shifts to No. 4. This process is repeated until the chimpanzee finally reaches the end door, which is the correct one, and receives some positive reinforcement.

The fourth phenomenon to be explained is the tendency of the animals to overshoot the mark in shifting toward the correct door when the correct door is the second one from the end. All of Spence's subjects exhibited this tendency. If an animal is shifting from left to right and the correct door is No. 4, next to the end door, No. 5, the chimpanzee reaches No. 4, is rewarded, but on the next try goes to No. 5 instead of staying at No. 4. Oddly enough, this is exactly what the theory would predict. Let us suppose that in the process of shifting from left to right the animal has reached No. 3. In addition to the inhibitory potential it acquired from the nonreinforced response to No. 2, No. 3 now has an inhibitory potential in its own right of, let us say, -10. This generalizes to No. 4 to the extent of, say, -6, and the gradient falls to zero at No. 5. The animal now makes a response to No. 4 and is reinforced. This gives No. 4 an excitatory potential of $+15$ and it generalizes to No. 3 and No. 5 to the extent of $+11$. The response tendencies after the reinforced response to No. 4 are thus:

	No. 3	No. 4	No. 5
No. 2 gradient	− 6	0	0
No. 3 gradient	−10	− 6	0
No. 4 gradient(+)	+11	+15	+11
Response tendencies	− 5	+ 9	+11

Hence it can be predicted in spite of the reinforcement at No. 4 that the next response will be to door No. 5. After a nonreinforced response to No. 5, door No. 4 will have the edge, and the animal will then make the correct response, for No. 3 will then have a resultant value of -5, the same as before; No. 4 will have $+3$ and No. 5 will be reduced to $+1$. As far as this phenomenon of overshooting the mark is concerned, the data seem to be adequately accounted for by the hypothesis.

While this gradient theory appears to have some explanatory value, and this paper has attempted to show that it has, it is not intended that this hypothesis be contradictory to all other theories of multiple-choice learning. It does, however, take the problem out of the realm of such concepts as ideation and link it to such other sorts of learning as conditioning and discrimination learning, and that much was intended. But this is not to say that this gradient theory is a complete theory of multiple-choice learning. Presumably, multiple-choice learning, like any other kind of

learning, is subject to such factors as drive strength, delay of reinforcement, amount of reward, etc. (3), and a more adequate theory than the present one will be a theory that synthesizes all of these things.

In summary, then, this gradient theory of multiple-choice learning assumes that responses are the outcome of excitation due to reinforcement and inhibition due to nonreinforcement. Both excitation and inhibition generalize in a gradient-like fashion, and they interact in such a way that the strength of the response tendency is the algebraic sum of excitation (positive) and inhibition (negative). On the basis of this theory, explanations are offered for four phenomena of multiple-choice learning: (*a*) the fact that animals find the middle-door problem harder than the end-door problem, (*b*) the fact that human subjects find it very difficult when the correct choice alternates about the middle, (*c*) the fact that in chimpanzees the shift from a formerly correct response to a new correct response is a gradual approach process, and (*d*) the fact that in so shifting, chimpanzees have a tendency to overshoot the mark and respond to the next door just after being rewarded at the correct door.

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MICE, MONKEYS, MEN, AND MOTIVES[1]

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Many of psychology's theoretical growing pains—or, in modern terminology, conditioned anxieties—stem from the behavioral revolution of Watson. The new psychology intuitively disposed of instincts and painlessly disposed of hedonism. But having completed this St. Bartholomew–type massacre, behavioristic motivation theory was left with an aching void, a nonhedonistic aching void, needless to say.

Before the advent of the Watsonian scourge the importance of external stimuli as motivating forces was well recognized. Psychologists will always remain indebted to Loeb's (21) brilliant formulation of tropistic theory, which emphasized, and probably overemphasized, the powerful role of external stimulation as the primary motivating agency in animal behavior. Unfortunately, Loeb's premature efforts to reduce all behavior to overly simple mathematical formulation, his continuous acceptance of new tropistic constructs in an effort to account for any aberrant behavior not easily integrated into his original system, and his abortive attempt to encompass all behavior into a miniature theoretical system doubtless led many investigators to underestimate the value of his experimental contributions.

Thorndike (30) was simultaneously giving proper emphasis to the role of external stimulation as a motivating force in learning and learned performances. Regrettably, these motivating processes were defined in terms of pain and pleasure, and it is probably best for us to dispense with such lax, ill-defined, subjective terms as pain, pleasure, anxiety, frustration, and hypotheses—particularly in descriptive and theoretical rodentology.

Instinct theory, for all its terminological limitations, put proper emphasis on the motivating power of external stimuli; for, as so brillantly described by Watson (31) in 1941, the instinctive response was elicited by "serial stimulation," much of which was serial external stimulation.

The almost countless researches on tropisms and instincts might well have been expanded to form a solid and adequate motivational theory for psychology—a theory with a proper emphasis on the role of the external stimulus and an emphasis on the importance of incentives as opposed to internal drives per se.

It is somewhat difficult to understand how this vast and valuable literature was to become so completely obscured and how the importance of the external stimulus as a motivating agent was to become lost. Pain-pleasure theory was discarded because the terminology had subjective, philosophical implications. Instinct theory fell into disfavor because psychologists rejected the dichotomized heredity-environment controversy and, also, because the term "instinct" had more than one meaning. Why tropistic theory disappeared remains a mystery, particularly inasmuch as most of the researches were carried out on subprimate animal forms.

Modern motivation theory apparently evolved from an overpopularization of certain experimental and theoretical ma-

[1] This paper was presented September 3, 1951, as the presidential address of the Division of Experimental Psychology at the Chicago meetings of the American Psychological Association.

terials. Jennings' (14) demonstration that "physiological state" played a role in determining the behavior of the lower animal was given exaggerated importance and emphasis, thereby relegating the role of external stimulation to a secondary position as a force in motivation. The outstanding work in the area of motivation between 1920 and 1930 related to visceral drives and drive cycles and was popularized by Richter's idealized theoretical paper on "Animal Behavior and Internal Drives" (26) and Cannon's *The Wisdom of the Body* (3).

When the self-conscious behavior theorists of the early thirties looked for a motivation theory to integrate with their developing learning constructs, it was only natural that they should choose the available tissue-tension hypotheses. Enthusiastically and uncritically the S–R theorists swallowed these theses whole. For fifteen years they have tried to digest them, and it is now time that these theses be subjected to critical examination, analysis, and evaluation. We do not question that these theses have fertilized the field of learning, but we do question that the plants that have developed are those that will survive the test of time.

It is my belief that the theory which describes learning as dependent upon drive reduction is false, that internal drive as such is a variable of little importance to learning, and that this small importance steadily decreases as we ascend the phyletic scale and as we investigate learning problems of progressive complexity. Finally, it is my position that drive-reduction theory orients learning psychologists to attack problems of limited importance and to ignore the fields of research that might lead us in some foreseeable future time to evolve a theoretical psychology of learning that transcends any single species or order.

There can be no doubt that the single-celled organisms such as the amoeba and the paramecium are motivated to action both by external and internal stimuli. The motivation by external stimulation gives rise to heliotropisms, chemotropisms, and rheotropisms. The motivation by internal stimulation produces characteristic physiological states which have, in turn, been described as chemotropisms. From a phylogenetic point of view, moreover, neither type of motive appears to be more basic or more fundamental than the other. Both types are found in the simplest known animals and function in interactive, rather than in dominant-subordinate, roles.

Studies of fetal responses in animals from opossum to man give no evidence suggesting that the motivation of physiological states precedes that of external incentives. Tactual, thermal, and even auditory and visual stimuli elicit complex patterns of behavior in the fetal guinea pig, although this animal has a placental circulation which should guarantee against thirst or hunger (4). The newborn opossum climbs up the belly of the female and into the pouch, apparently in response to external cues; if visceral motives play any essential role, it is yet to be described (20). The human fetus responds to external tactual and nociceptive stimuli at a developmental period preceding demonstrated hunger or thirst motivation. Certainly, there is no experimental literature to indicate that internal drives are ontogenetically more basic than exteroceptive motivating agencies.

Tactual stimulation, particularly of the cheeks and lips, elicits mouth, head, and neck responses in the human neonate, and there are no data demonstrating that these responses are conditioned, or even dependent, upon physiological drive states. Hunger appears to lower the threshold for these responses to tactual stimuli. Indeed, the main role of

the primary drive seems to be one of altering the threshold for precurrent responses. Differentiated sucking response patterns have been demonstrated to quantitatively varied thermal and chemical stimuli in the infant only hours of age (15), and there is, again, no reason to believe that the differentiation could have resulted from antecedent tissue-tension reduction states. Taste and temperature sensations induced by the temperature and chemical composition of the liquids seem adequate to account for the responses.

There is neither phylogenetic nor ontogenetic evidence that drive states elicit more fundamental and basic response patterns than do external stimuli; nor is there basis for the belief that precurrent responses are more dependent upon consummatory responses than are consummatory responses dependent upon precurrent responses. There is no evidence that the differentiation of the innate precurrent responses is more greatly influenced by tissue-tension reduction than are the temporal ordering and intensity of consummatory responses influenced by conditions of external stimulation.

There are logical reasons why a drive-reduction theory of learning, a theory which emphasizes the role of internal, physiological-state motivation, is entirely untenable as a motivational theory of learning. The internal drives are cyclical and operate, certainly at any effective level of intensity, for only a brief fraction of any organism's waking life. The classical hunger drive physiologically defined ceases almost as soon as food—or nonfood—is ingested. This, as far as we know, is the only case in which a single swallow portends anything of importance. The temporal brevity of operation of the internal drive states obviously offers a minimal opportunity for conditioning and a maximal opportunity for extinction. The human being, at least in the continental United States, may go for days or even years without ever experiencing true hunger or thirst. If his complex conditioned responses were dependent upon primary drive reduction, one would expect him to regress rapidly to a state of tuitional oblivion. There are, of course, certain recurrent physiological drive states that are maintained in the adult. But the studies of Kinsey (17) indicate that in the case of one of these there is an inverse correlation between presumed drive strength and scope and breadth of learning, and in spite of the alleged reading habits of the American public, it is hard to believe that the other is our major source of intellectual support. Any assumption that derived drives or motives can account for learning in the absence of primary drive reduction puts an undue emphasis on the strength and permanence of derived drives, at least in subhuman animals. Experimental studies to date indicate that most derived drives (24) and second-order conditioned responses (25) rapidly extinguish when the rewards which theoretically reduce the primary drives are withheld. The additional hypothesis of functional autonomy of motives, which could bridge the gap, is yet to be demonstrated experimentally.

The condition of strong drive is inimical to all but very limited aspects of learning—the learning of ways to reduce the internal tension. The hungry child screams, closes his eyes, and is apparently oblivious to most of his environment. During this state he eliminates response to those aspects of his environment around which all his important learned behaviors will be based. The hungry child is a most incurious child, but after he has eaten and become thoroughly sated, his curiosity and all the learned responses associated with his curiosity take place. If this learning is conditioned to an internal drive state,

we must assume it is the resultant of backward conditioning. If we wish to hypothesize that backward conditioning is dominant over forward conditioning in the infant, it might be possible to reconcile fact with S–R theory. It would appear, however, that alternate theoretical possibilities should be explored before the infantile backward conditioning hypothesis is accepted.

Observations and experiments on monkeys convinced us that there was as much evidence to indicate that a strong drive state inhibits learning as to indicate that it facilitates learning. It was the speaker's feeling that monkeys learned most efficiently if they were given food before testing, and as a result, the speaker routinely fed his subjects before every training session. The rhesus monkey is equipped with enormous cheek pouches, and consequently many subjects would begin the educational process with a rich store of incentives crammed into the buccal cavity. When the monkey made a correct response, it would add a raisin to the buccal storehouse and swallow a little previously munched food. Following an incorrect response, the monkey would also swallow a little stored food. Thus, both correct and incorrect responses invariably resulted in S–R theory drive reduction. It is obvious that under these conditions the monkey cannot learn, but the present speaker developed an understandable skepticism of this hypothesis when the monkeys stubbornly persisted in learning, learning rapidly, and learning problems of great complexity. Because food was continuously available in the monkey's mouth, an explanation in terms of differential fractional anticipatory goal responses did not appear attractive. It would seem that the Lord was simply unaware of drive-reduction learning theory when he created, or permitted the gradual evolution of, the rhesus monkey.

The langurs are monkeys that belong to the only family of primates with sacculated stomachs. There would appear to be no mechanism better designed than the sacculated stomach to induce automatically prolonged delay of reinforcement defined in terms of homeostatic drive reduction. Langurs should, therefore, learn with great difficulty. But a team of Wisconsin students has discovered that the langurs in the San Diego Zoo learn at a high level of monkey efficiency. There is, of course, the alternative explanation that the inhibition of hunger contractions in multiple stomachs is more reinforcing than the inhibition of hunger contractions in one. Perhaps the quantification of the gastric variable will open up great new vistas of research.

Actually, the anatomical variable of diversity of alimentary mechanisms is essentially uncorrelated with learning to food incentives by monkeys and suggests that learning efficiency is far better related to tensions in the brain than in the belly.

Experimental test bears out the fact that learning performance by the monkey is unrelated to the theoretical intensity of the hunger drive. Meyer (23) tested rhesus monkeys on discrimination-learning problems under conditions of maintenance-food deprivation of 1.5, 18.5, and 22.5 hours and found no significant differences in learning or performance. Subsequently, he tested the same monkeys on discrimination-reversal learning following 1, 23, and 47 hours of maintenance-food deprivation and, again, found no significant differences in learning or in performance as measured by activity, direction of activity, or rate of responding. There was some evidence, not statistically significant, that the most famished subjects were a bit overeager and that in-

tense drive exerted a mildly inhibitory effect on learning efficiency.

Meyer's data are in complete accord with those presented by Birch (1), who tested six young chimpanzees after 2, 6, 12, 24, and 48 hr. of food deprivation and found no significant differences in proficiency of performance on six patterned string problems. Observational evidence led Birch to conclude that intense food deprivation adversely affected problem solution because it led the chimpanzee to concentrate on the goal to the relative exclusion of the other factors.

It may be stated unequivocally that, regardless of any relationship that may be found for other animals, there are no data indicating that intensity of drive state and the presumably correlated amount of drive reduction are positively related to learning efficiency in primates.

In point of fact there is no reason to believe that the rodentological data will prove to differ significantly from those of monkey, chimpanzee, and man. Strassburger (29) has recently demonstrated that differences in food deprivation from 5 hours to 47 hours do not differentially affect the habit strength of the bar-pressing response as measured by subsequent resistance to extinction. Recently, Sheffield and Roby (28) have demonstrated learning in rats in the absence of primary drive reduction. Hungry rats learned to choose a maze path leading to a saccharin solution, a nonnutritive substance, in preference to a path leading to water. No study could better illustrate the predominant role of the external incentive–type stimulus on the learning function. These data suggest that, following the example of the monkey, even the rats are abandoning the sinking ship of reinforcement theory.

The effect of intensity of drive state on learning doubtless varies as we ascend the phyletic scale and certainly varies, probably to the point of almost complete reversal, as we pass from simple to complex problems, a point emphasized some years ago in a theoretical article by Maslow (22). Intensity of nociceptive stimulation may be positively related to speed of formation of conditioned avoidance responses in the monkey, but the use of intense nociceptive stimulation prevents the monkey from solving any problem of moderate complexity. This fact is consistent with a principle that was formulated and demonstrated experimentally many years ago as the Yerkes-Dodson law (32). There is, of course, no reference to the Yerkes-Dodson law by any drive-reduction theorist.

We do not mean to imply that drive state and drive-state reduction are unrelated to learning; we wish merely to emphasize that they are relatively unimportant variables. Our primary quarrel with drive-reduction theory is that it tends to focus more and more attention on problems of less and less importance. A strong case can be made for the proposition that the importance of the psychological problems studied during the last fifteen years has decreased as a negatively accelerated function approaching an asymptote of complete indifference. Nothing better illustrates this point than the kinds of apparatus currently used in "learning" research. We have the single-unit T-maze, the straight runway, the double-compartment grill box, and the Skinner box. The single-unit T-maze is an ideal apparatus for studying the visual capacities of a nocturnal animal; the straight runway enables one to measure quantitatively the speed and rate of running from one dead end to another; the double-compartment grill box is without doubt the most efficient torture chamber which is still legal; and the Skinner box enables one to demonstrate discrimination learning in a greater number of trials than is required by any

other method. But the apparatus, though inefficient, give rise to data which can be splendidly quantified. The kinds of learning problems which can be efficiently measured in these apparatus represent a challenge only to the decorticate animal. It is a constant source of bewilderment to me that the neobehaviorists who so frequently belittle physiological psychology should choose apparatus which, in effect, experimentally decorticate their subjects.

The Skinner box is a splendid apparatus for demonstrating that the rate of performance of a learned response is positively related to the period of food deprivation. We have confirmed this for the monkey by studying rate of response on a modified Skinner box following 1, 23, and 47 hr. of food deprivation. Increasing length of food deprivation is clearly and positively related to increased rate of response. This functional relationship between drive states and responses does not hold, as we have already seen, for the monkey's behavior in discrimination learning or in acquisition of any more complex problem. The data, however, like rat data, are in complete accord with Crozier's (6) finding that the acuteness of the radial angle of tropistic movements in the slug Limax is positively related to intensity of the photic stimulation. We believe there is generalization in this finding, and we believe the generalization to be that the results from the investigation of simple behavior may be very informative about even simpler behavior but very seldom are they informative about behavior of greater complexity. I do not want to discourage anyone from the pursuit of the psychological Holy Grail by the use of the Skinner box, but as far as I am concerned, there will be no moaning of farewell when we have passed the pressing of the bar.

In the course of human events many psychologists have children, and these children always behave in accord with the theoretical position of their parents. For purposes of scientific objectivity the boys are always referred to as "Johnny" and the girls as "Mary." For some eleven months I have been observing the behavior of Mary X. Perhaps the most striking characteristic of this particular primate has been the power and persistence of her curiosity-investigatory motives. At an early age Mary X demonstrated a positive valence to parental thygmotatic stimulation. My original interpretation of these tactual-thermal erotic responses as indicating parental affection was dissolved by the discovery that when Mary X was held in any position depriving her of visual exploration of the environment, she screamed; when held in a position favorable to visual exploration of the important environment, which did not include the parent, she responded positively. With the parent and position held constant and visual exploration denied by snapping off the electric light, the positive responses changed to negative, and they returned to positive when the light was again restored. This behavior was observed in Mary X, who, like any good Watson child, showed no "innate fear of the dark."

The frustrations of Mary X appeared to be in large part the results of physical inability to achieve curiosity-investigatory goals. In her second month, frustrations resulted from inability to hold up her head indefinitely while lying prone in her crib or on a mat and the consequent loss of visual curiosity goals. Each time she had to lower her head to rest, she cried lustily. At nine weeks attempts to explore (and destroy) objects anterior resulted in wriggling backward away from the lure and elicited violent negative responses. Once she negotiated forward locomotion, exploration set in, in earnest, and, much

to her parents' frustration, shows no sign of diminishing.

Can anyone seriously believe that the insatiable curiosity-investigatory motivation of the child is a second-order or derived drive conditioned upon hunger or sex or any other internal drive? The S–R theorist and the Freudian psychoanalyst imply that such behaviors are based on primary drives. An informal survey of neobehaviorists who are also fathers (or mothers) reveals that all have observed the intensity and omnipresence of the curiosity-investigatory motive in their own children. None of them seriously believes that the behavior derives from a second-order drive. After describing their children's behavior, often with a surprising enthusiasm and frequently with the support of photographic records, they trudge off to their laboratories to study, under conditions of solitary confinement, the intellectual processes of rodents. Such attitudes, perfectly in keeping with drive-reduction theory, no doubt account for the fact that there are no experimental or even systematic observational studies of curiosity-investigatory–type external-incentive motives in children.

A key to the real learning theory of any animal species is knowledge of the nature and organization of the unlearned patterns of response. The differences in the intellectual capabilities of cockroach, rat, monkey, chimpanzee, and man are as much a function of the differences in the inherent patterns of response and the differences in the inherent motivational forces as they are a function of sheer learning power. The differences in these inherent patterns of response and in the motivational forces will, I am certain, prove to be differential responsiveness to external stimulus patterns. Furthermore, I am certain that the variables which are of true, as opposed to psychophilosophical, importance are not constant from learning problem to learning problem even for the same animal order, and they are vastly diverse as we pass from one animal order to another.

Convinced that the key to human learning is not the conditioned response but, rather, motivation aroused by external stimuli, the speaker has initiated researches on curiosity-manipulation behavior as related to learning in monkeys (7, 10, 12). The justification for the use of monkeys is that we have more monkeys than children. Furthermore, the field is so unexplored that a systematic investigation anywhere in the phyletic scale should prove of methodological value. The rhesus monkey is actually a very incurious and nonmanipulative animal compared with the anthropoid apes, which are, in turn, very incurious nonmanipulative animals compared with man. It is certainly more than coincidence that the strength and range of curiosity-manipulative motivation and position within the primate order are closely related.

We have presented three studies which demonstrate that monkeys can and do learn to solve mechanical puzzles when no motivation is provided other than presence of the puzzle. Furthermore, we have presented data to show that once mastered, the sequence of manipulations involved in solving these puzzles is carried out relatively flawlessly and extremely persistently. We have presented what we believe is incontrovertible evidence against a second-order drive interpretation of this learning.

A fourth study was carried out recently by Gately at the Wisconsin laboratories. Gately directly compared the behavior of two groups of four monkeys presented with banks of four identical mechanical puzzles, each utilizing three restraining devices. All four food- plus puzzle-rewarded monkeys solved the four identical puzzles, and only one of the four monkeys motivated by curiosity

alone solved all the puzzles. This one monkey, however, learned as rapidly and as efficiently as any of the food-rewarded monkeys. But I wish to stress an extremely important observation made by Gately and supported by quantitative records. When the food-rewarded monkeys had solved a puzzle, they abandoned it. When the nonfood-rewarded animals had solved the puzzle, they frequently continued their explorations and manipulations. Indeed, one reason for the nonfood-rewarded monkeys' failure to achieve the experimenter's concept of solution lay in the fact that the monkey became fixated in exploration and manipulation of limited puzzle or puzzle-device components. From this point of view, hunger-reduction incentives may be regarded as motivation-destroying, not motivation-supporting, agents.

Twenty years ago at the Vilas Park Zoo, in Madison, we observed an adult orangutan given two blocks of wood, one with a round hole, one with a square hole, and two plungers, one round and one square. Intellectual curiosity alone led it to work on these tasks, often for many minutes at a time, and to solve the problem of inserting the round plunger in both holes. The orangutan never solved the problem of inserting the square peg into the round hole, but inasmuch as it passed away with perforated ulcers a month after the problem was presented, we can honestly say that it died trying. And in defense of this orangutan, let it be stated that it died working on more complex problems than are investigated by most present-day learning theorists.

Schiller [2] has reported that chimpanzees solve multiple-box–stacking problems without benefit of food rewards, and he has presented observational evidence that the joining of sticks resulted from manipulative play responses.

[2] Personal communication.

The Cebus monkey has only one claim to intellectual fame—an ability to solve instrumental problems that rivals the much publicized ability of the anthropoid apes (11, 18). It can be no accident that the Cebus monkey, inferior to the rhesus on conventional learning tasks, demonstrates far more spontaneous instrumental-manipulative responses than any old-world form. The complex, innate external-stimulus motives are variables doubtlessly as important as, or more important than, tissue tensions, stimulus generalization, excitatory potential, or secondary reinforcement. It is the oscillation of sticks, not cortical neurons, that enables the Cebus monkey to solve instrumental problems.

No matter how important may be the analysis of the curiosity-manipulative drives and the learning which is associated with them, we recognize the vast and infinite technical difficulties that are inherent in the attack on the solution of these problems—indeed, it may be many years before we can routinely order such experiments in terms of latin squares and factorial designs, the apparent *sine qua non* for publication in the *Journal of Experimental Psychology* and the *Journal of Comparative and Physiological Psychology*.

There is, however, another vast and important area of external-stimulus incentives important to learning which has been explored only superficially and which can, and should, be immediately and systematically attacked by rodentologists and primatologists alike. This is the area of food incentives—or, more broadly, visuo-chemo variables—approached from the point of view of their function as motivating agents per se. This function, as the speaker sees it, is primarily an affective one and only secondarily one of tissue-tension reduction. To dispel any fear of subjectivity, let us state that the affective tone of food incentives can probably be scaled

by preference tests with an accuracy far exceeding any scaling of tissue tensions. Our illusion of the equal-step intervals of tissue tensions is the myth that length of the period of deprivation is precisely related to tissue-tension intensity, but the recent experiments by Koch and Daniel (19) and Horenstein (13) indicate that this is not true, thus beautifully confirming the physiological findings of thirty years ago.

Paired-comparison techniques with monkeys show beyond question that the primary incentive variables of both differential quantity and differential quality can be arranged on equal-step scales, and there is certainly no reason to believe that variation dependent upon subjects, time, or experience is greater than that dependent upon physiological hunger.

In defense of the rat and its protagonists, let it be stated that there are already many experiments on this lowly mammal which indicate that its curiosity-investigatory motives and responsiveness to incentive variables can be quantitatively measured and their significant relationship to learning demonstrated. The latent learning experiments of Buxton (2), Haney (9), Seward, Levy, and Handlon (27), and others have successfully utilized the exploratory drive of the rat. Keller (16) and Zeaman and House (35) have utilized the rat's inherent aversion to light, or negative heliotropistic tendencies, to induce learning. Flynn and Jerome (8) have shown that the rat's avoidance of light is an external-incentive motivation that may be utilized to obtain the solution of complex learned performances. For many rats it is a strong and very persistent form of motivation. The importance of incentive variables in rats has been emphasized and re-emphasized by Young (33), and the influence of incentive variables on rat learning has been demonstrated by Young (33), Zeaman (34), Crespi (5), and others. I am not for one moment disparaging the value of the rat as a subject for psychological investigation; there is very little wrong with the rat that cannot be overcome by the education of the experimenters.

It may be argued that if we accept the theses of this paper, we shall be returning to an outmoded psychology of tropisms, instincts, and hedonism. There is a great deal of truth to this charge. Such an approach might be a regression were it not for the fact that psychology now has adequate techniques of methodology and analysis to attack quantifiably these important and neglected areas. If we are ever to have a comprehensive theoretical psychology, we must attack the problems whose solution offers hope of insight into human behavior, and it is my belief that if we face our problems honestly and without regard to, or fear of, difficulty, the theoretical psychology of the future will catch up with, and eventually even surpass, common sense.

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THE PLACE OF THEORY IN SCIENCE [1]

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I was led to select "The Place of Theory in Science" as the subject of my address this evening by observations made last year as I listened with you to our divisional symposium upon "Theories of Learning." On that occasion every speaker ardently defended a theory which he had himself espoused, marshalling evidence favorable to his own views, and attacking the proposals of his opponents. It was a lively and heated discussion, but its fervor was worthy of a better cause.

Consider some of the theories "ardently defended" in our youth: those pertaining to the "mind-body" relationship; those dealing with "play," where Herbert Spencer, Karl Gross, John Ruskin, and G. Stanley Hall were now extolled and again derided; and those concerned with the origin of language.

The theories of language naïvely discussed included the onomatopoeic or "bow-wow theory" which attributes the origin of language to the attempt of our primitive ancestors to imitate sounds heard and to use those imitations to represent the objects of their origin; the interjectional theory, called in derision by its opponents the "pooh-pooh theory," which supposes that language arose from the automatic exclamations or ejaculations of primitive man; the reflex theory, nicknamed the "ding-dong theory," which holds that various objects and situations in man's environment arouse unlearned or reflex gestures and vocalizations which, under repetition, become associated with them and ultimately to represent them; the social or "yo-he-ho theory" which maintains that language developed from vocalizations made by primitive men in the course of cooperative activities; and finally the theory which regards language as a form of expressive movement that accompanied and arose from gesture.

We were, of course, also conversant with all the classical and the newer theories of sensation, perception, feeling, emotion, attention, and the like, which have come down to the present. I mentioned in particular the mind-body theories and those of play and language because few students today are concerned with them and many have not even heard of them. They were, however, argued and championed in our youth as ardently as any theory today —even learning theory. The terse and uncomplimentary nicknames given the various theories of language impart, for example, the intensity of the feelings of the participants in that controversy.

What has become of those early youthful debates? Did criticism prove a leaven to bring about unanimity of opinion upon theory? Not a bit of it. The mind-body problem was not solved; it merely passed from the psychological scene. It was given over to philosophy and metaphysics and the doctrine of psychophysical parallelism was tacitly assumed—at least by many experimental psychologists, who, in order to get on with their proper work, resolved the problem by placing parallelism among their postulates. Like a fortified atoll, which has long resisted frontal attack, the mind-body problem was by-passed and when that was done, it developed that it need not be taken. Similarly, the origins of play and of language are no longer of concern. Their existence is self-evident and their study need not

[1] Address of the retiring president of the Division of General Psychology, American Psychological Association, Chicago, Illinois, August 31, 1951.

be delayed, as it was eventually discovered, for a verdict regarding their origin —which is still, after nearly fifty years, to be returned. The theories of black and of the elementariness of the intermediate colors, also long and vigorously debated, have similarly disappeared. Indeed, though I have searched long through the history of our science for a theory that gave way to criticism, I have been unable to find one.

Theories do not succumb to abstract argumentation. They do not die but—like an occasional old soldier—they just fade away. They pass from the scientific stage not because they have been discredited but because they have been superseded or by-passed—pushed off the stage and replaced by other theories.

Why then become enamored with theory and argue and debate it emotionally as if its issues were of vital concern? The chief and, as I believe, the only justifiable reasons are pride and the obligations of authorship. The author of a theory owes allegiance to his brain-child as to the child of his body. He is responsible for its elucidation and clarification, hence may be pardoned if he is emotional in its defense and now and then sarcastic and caustic in his reply—even as he would be in case his children were unjustly criticized (as he must believe) by a disagreeable (as he must think) neighbor. If he were wise, he might concede that his theory and even his children need correction. Some authors and parents are of sufficient stature to grant that and they modify their theories and the behavior of their children in the light of criticism received, but their number is few. Most authors defend their theories to the last jot and tittle; and most parents admit of no flaws, at least not in the face of criticism, among their "little angels."

If authors of theories were the only ones to become emotional in defense, the reasons for their fervor would not be far to seek, but others, who espouse and defend theory, are often as ardent and as zealous as the authors themselves. This may be due, as I have suggested, to youthful loyalties, or to the desire to hold fast to something in this rapidly changing and fluctuating world, even if it be merely theory. In either case, however, it is a mistake because there is such a thing as theory-blindness, i.e., being so blinded by adherence to a specific theory as to be unable truly to see the facts. When one defends theory, particularly if one is emotional about it, one is very apt to select the data which support it and be blind to all others that do not. Nowhere is this better shown than in the conflict between the Helmholtz and the Hering theories of vision. The results of experiments in vision, particularly upon color-blindness, color-mixing, and color zones, are determined by the theoretical prejudices of the investigators.

Owing to the training that students of my generation received, my loyalties among the various contending theories were early established. Fortunately for me, however, the exalted opinion that I had of theory in general and of my favorite theories in particular did not last for long. At my first conference as a graduate student with Titchener, I had the temerity to ask him in which theory of vision he believed. He replied, "Believe, believe, why, I don't believe in any." Then followed a discourse upon theory and its place in experiment which ended with the admonition, "Carry your theories lightly," by which he meant do not rush to their attack or to their defense but go about your own proper work of gathering facts. They and they alone verify or disprove theory.

This advice does not mean, of course, that theory should be avoided and not discussed nor criticized. Quite to the contrary, it means that every one within

his own special field of knowledge should know all the theories, love a few, wed none. The best way to learn to know a theory is to discuss it but keep the discussion centered upon fact. Keep your theories fluid; do not permit them to crystallize until they have been factually demonstrated or disproved.

Yes, carry theory lightly that you may follow where fact may lead; and that you may forsake a theory without suffering feelings of guilt. Free from those paralyzing inhibitions, you may discuss, criticize, and judge theory impartially, which is something you will never be able to do as long as you carry the defense of theory, like Sinbad's Old Man of the Sea, upon your shoulders. Theory takes over, if given the opportunity, and you may not be so fortunate as Sinbad in freeing yourself from that incubus once you have burdened yourself with it.

Controversy over theory is, moreover, futile. It advertises and calls attention to the theory criticized and it is not effective against the will to believe. It is useless, bootless, and to no purpose. History is replete with instances in which that is shown to be true.

Sir William Hamilton, for example, spent many of the best years of his life performing crucial experiments and writing polemical articles against phrenology. Did his studies and articles do any good? Did they hasten the demise of phrenology? Did they wean any one away from those false doctrines? Was Hamilton's time and labor spent to the best advantage? The answer to these questions must be "no." Phrenology endured as a controversial doctrine nearly forty years after Hamilton had written his last critical paper. It was the growth of knowledge; the results of the experimental work of the brain physiologists, Broca, Fritsch, Hitzig, Ferrier, Goltz, Munk, that assigned the doctrine of phrenology to yesterday's seven thousand years.

Again, for nearly fifty years after the appearance of the *Origin of Species* (1859) the biologists and churchmen were engaged in a bitter controversy over Darwin's theories and the dogmas of revealed religion. The time and effort spent in those debates was wasted. The bishops could not convince the biologists and the biologists could not convince the bishops. The debates were destined to be futile from the start. The bishops were loyal to their dogma and the facts and arguments of the biologists fell upon deaf ears; and the scientists were loyal to Darwin's theory and the dogma of the churchmen left them unmoved. If the biologists had carried their theories lightly, had ignored the churchmen's criticisms, had gone about their own proper business of experimenting and seeking facts, their science would not have been relatively sterile experimentally during the forty-year period following the appearance of Darwin's book. Mendel did just that. He carried on within the precincts of a monastery and gave us results of first magnitude which were overlooked and neglected in the intensity of the controversy being waged over theory. It was not until 1900, when Mendel's experiments and results were discovered and publicized by De Vries, that the biologists again went about their own proper work.

Criticism of Freudianism is, similarly, labor lost. We have had a tremendous amount of it during the last forty years but it has been futile. It is not read by those wedded to psychoanalytic dogma and it is not needed by those who are not.

Thus far in my discussion I have been using the term "theory" in its broad, general sense to denote any tentative explanation of phenomena. In addition to that meaning, "theory" has a restricted use. It is frequently limited to propositions that have received a considerable amount of verification, that have passed beyond the tentative stage. When that is the case, its broad, general meaning is given over to such terms as ***conjecture, speculation, doctrine, hypothesis.***

Attempts have been made to define all of these terms rigidly and to arrange them in

a progressive series according to the increasing adequacy of the evidence for them. Beginning with *conjecture* (an inference drawn wholly from presumptive evidence), and going to *speculation* (a generalization suggested by available but insecure observations), to *doctrine* (a thesis advanced upon the basis of authority), to *hypothesis* (a preliminary assumption or principle adopted for the explanation of observed facts), to *theory* (a specific formula propounded on the basis of secure facts for the purpose of explaining a group of given phenomena), and on to *law* (a statement of the fundamental uniformity of the facts of observation), the evidence for each concept becomes more and more factual and secure. As desirable as the rigid definition and serial ordering of these terms may be, the attempts made to achieve them have not been accepted. These terms, particularly hypothesis and theory, are used loosely and indiscriminately. What one author calls a "theory" another calls an "hypothesis" and frequently you find indecision and an author writing of "theory or hypothesis."

Among these various terms and definitions, two differences appear that are aptly illustrated by the figures of speech used to denote their function. One is that they are "bridges," spanning gaps in our knowledge, which are constructed to bring together large groups of facts or phenomena. The other is that they are "tools" devised for and used on specific occasions, being restricted to the explanation of a small number or groups of facts.

The first, the "bridge" analogy, is amply illustrated by such examples as the nebular hypothesis, the doctrine of associationism, the doctrine of specific nerve energies, the doctrine of creative synthesis, the phi-gamma hypothesis. Though bridges may be small, like culverts under a road, we usually think of them as grand, majestic spans over vast tracts. For such conceptual structures, I am, following Newton's lead, using "hypothesis." Newton restricted this term to the larger explanations, because etymologically "hypothesis" means "foundation."

The second, the "tool" analogy, is illustrated by reference to any well-reported experiment. It is the specific proposition the experimenter had in mind when he designed his investigation. It may be large, like a crane or derrick, or small, like a tack hammer or needle. In either case, it has a specific purpose, i.e., the formulation or tentative statement of the problem which the study was undertaken to test. For such concepts I am using the term "theory."

Since bridges and tools may be large or small, there may be times when they overlap in size, when the analogies do not hold —a crane, a large tool, may, for example, be larger than a culvert, a small bridge. By and large, however, a bridge exceeds a tool in size and extent, hence the distinction, since we are dealing only with analogies, need not be strained.

As tools are used in the construction, maintenance, and continuation of bridges, so theories are deducible from and subsumed by hypotheses. It is always with reference to some larger hypothesis that theories are formulated and employed.

Hypotheses, in the "bridge" sense, though regarded lightly by every one, are of great importance to science. They not only guide and give sanction to theory and experiment but they also consolidate the factual gains as they are made by giving them a basis of classification. The last, a basis of classification, is by no means the least of their contributions though it is frequently overlooked and ignored by writers upon this topic. The Müllerian doctrine of the specific energy of nerves is a case in point. As I have once before pointed out (4), through the ages since Aristotle, various and numerous investigators have sought, under the traditional doctrine of the five senses, to analyze the fifth sense, the sense of touch, into several senses. Their efforts, in many instances surprisingly accurate, were, however, without lasting significance. Their achievements made little impression upon their own age and generation and were unknown to succeeding generations. Every au-

thor wrote, against the background of the traditional doctrine of the five senses, as if he were proposing something new. Lapses of centuries and differences in language do not account for the lack of continuity for Bell, Erasmus Darwin's contemporary in England, gave no indication of knowing that Darwin had proposed a "muscle sense" when he wrote of it. What was lacking all along to consolidate the gains as they were being made through the centuries was the positive principle of an hypothesis. Müller, though he himself held to the traditional doctrine of the five senses, gave it to us in his principle of the dependence of qualitative difference upon the specific energy of nerves.

We should of course prefer to work with hypotheses that turn out to be true, but any hypothesis, true or false, that is capable of being tested by experiment is better than none. If false, it may lead to the formulation of a better, which in turn may lead to one still better, and thus, by successive demolitions and reconstructions, an accurate hypothesis—true knowledge—is approached.

The recognition of the importance of hypotheses has led to attempts to safeguard them, i.e., to draw up rigid rules regarding the legitimacy of their formulation. Thus, an hypothesis is legitimate scientifically only when the phenomena to be explained actually exist. This precept, that you must first establish your fact before you conjecture its causes, may seem elemental, but history yields numerous instances of its violation. Poor, lonely, wretched, and doubtlessly insane, old women were tried and executed in this country not so many years ago upon the assumption, that is, upon the hypothesis, that they were witches. We need not be smug and feel that our times are superior for some of us today speculate about modern "witches" which, though dressed in new garb, have no greater existence in fact than the old. For example, instead of telepathy we have ESP; instead of a wide variety of faculties or powers we have censor, ego, superego, and id.

I could continue, as can you, with a host of examples of the need for this rule, but I shall let one more, an historical and an amusing example, suffice.

Charles II of England, soon after the Royal Society was established under his patronage, sent it a request to explain the following phenomenon. "When a live fish is thrown into a basin of water, the basin, water, and fish do not weigh more than the basin and water before the fish is thrown in; whereas, when a dead fish is employed, the weight of the whole is exactly equal to the added weights of the basin, the water, and the fish" (5). To answer their king and patron, the members proceeded to study the problem and much learned discussion ensued. Elaborate papers, propounding various hypotheses, were read in explanation. At length a member, not satisfied with any of the hypotheses proposed, suggested that it might be expedient to see whether the facts were as the king had stated them. For this he was roundly condemned. Some members asserted that the "facts" were of such common knowledge that their test was not needed, and others declared that to doubt the "facts" was an insult to his majesty and to perform the experiment would be tantamount to an act of treason. Despite these objections, the experiment was made—when lo! to the confusion of the wise men of Gotham—the name by which the Society was then popularly known—it was found that the weight of the three, the basin, water, and fish, was identical whether a dead or a live fish was used.

A second rule regarding the legitimacy of an hypothesis is that it must not be barren, i.e., incapable of being subjected to the test of experiment, of being proved or disproved. Such hypotheses are of no use to science as they do not advance knowledge one iota. It now

and then turns out, however, that what is barren at one day and generation is not at the next when suitable instruments have been invented and adequate procedures devised. Thus, for example, most of the early hypotheses relating to air were barren and remained so until the air-pump was invented and the chemists had devised suitable methods of analysis. Since an hypothesis barren today may be fruitful tomorrow, much of the force of this second rule is lost.

Other rules governing the legitimacy of hypotheses are: they should not be at variance with known facts; they should not subvert that for which they were devised to explain; they should deal with real and not with hypothetical facts; they should not transcend experience; they should work simply and naturally and not involve the occult or supernatural in their explanations.

Though all of these rules are sound in the abstract, they have little practical value, in part because they are not widely known nor widely accepted, and in part because of the general belief that any rule which discourages hypothesis is undesirable.

Because we have a plethora of hypotheses in psychology, I sometimes sigh and wish that the rules governing their legitimacy were more widely known and strictly enforced. When I recall, however, what has happened in countries where a censorship has been applied to the hypotheses of science, I am content to let free enterprise reign. Hypotheses will never become a burden if we carry them lightly. We have the cure for the "disease" in our own hands.

Theory in the narrow, the "tool" sense, is the immediate basis of experimentation. It is the specific formulation of the problem—the overt reason for undertaking the work. By means of it the experiment is designed.

I was once charged and roundly criticized for undertaking an experiment "to support a theory" (6). Except for the verb "support," for which I substituted "test," I admitted the charge as I did not then and do not now see how an experimental problem can even be formulated, to say nothing of conducted and completed, without some sort of preceding theory (3).

Galilei's apocryphal experiment upon the velocity of falling bodies furnishes an apt illustration. According to report it was made to test the Aristotelian doctrine, which was widely accepted for many centuries, that heavy bodies fell faster than light bodies.

Galilei, as you will recall, is reported to have dropped simultaneously three bodies of widely varying weights from the top of the Leaning Tower of Pisa and to have found that they reached the ground, not in order of their weights, the heaviest first and the lightest last, but at the same time. We can only guess at what the theory was that prompted this experiment for neither he nor any of his contemporaries either reported or described this experiment (2).

Whatever our guess may be, the paradigm of a scientific experiment is that theory, under an inclusive hypothesis, comes before the mechanical work of collecting and treating the data. An experiment in science is the integration of these aspects. Unfortunately that is not always the case in psychology, for as Bentley has pointed out in his article on "The nature and uses of experiment in psychology" (1), the theoretical setting, the planning and arranging an event and its conditions, ranks high among the plural meanings attached today to an experiment within psychology. Bentley distinguishes no less than eight uses, the first five of which are without the benefit or sanction of a theory. Doing something, without a theory, is not

a scientific experiment. It is mere busy work. It contributes nothing to knowledge; it has no scientific purpose nor meaning. We thus escape the necessity, when such a report appears in print to clutter up our scientific literature, of taking it into account for, "if there is no meaning," as the king said to Alice in Wonderland, "that saves a world of trouble, you know, as we needn't try to find any."

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FACTS, PHENOMENA, AND FRAMES OF REFERENCE IN PSYCHOLOGY [1]

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A recent text book in psychology affirms boldly that facts, which are *true* in one frame of reference are *false* in another. "A fact, we find, is not an independent thing that we can memorize and depend upon and know that it will always be true. It is true only in its frame of reference, which means that it is false in others" (6, p. 4). This assertion is based on an idea now widely accepted and one which I have myself discussed in earlier papers, namely, that a fact is such only in a frame of reference (3, 4). In this paper I wish to examine the meaning of the statement "a fact is such only in a frame of reference" in order to bring out some ambiguities and clarify what I think is a basic misconception in Snygg and Combs. This task will also involve a discussion of frames of reference as well as the problem of the criteria for selection among such frames. The basic difficulty, the recognition of which inspires this study, is this: If facts are such only in a frame of reference, then we are confronted with a peculiar situation. In the first place no fact could occur which could cause a given frame of reference to be rejected. This is evident when we consider what a "fact" is. A fact, it is said, is the result of the interpretation of sense data by means of a conceptual scheme. The meaning of "conceptual scheme" is close enough to that of "frame of reference" to allow me to use the two interchangeably. A frame of reference involves a standpoint or a point of view from which phenomena are observed and in terms of which they are described. If the fact is the interpreted phenomenon, then clearly *only* those facts can occur which result when phenomena are described in the language of the given frame of reference. It becomes meaningless to say, as do Snygg and Combs, that science is seeking to sum up ". . . new facts inexplicable in the old frames of reference" (6). If no criteria exist for selecting among alternative frames of reference, then any frame of reference is of equal worth to any other frame and we have no way of deciding which to accept. Such a state of affairs would mean, e.g., that a Hullian frame of reference and a Freudian are matters of arbitrary choice. The suggestion at once arises, "Why not a complete relativism of such frames?" If the principle of relativity is to be applied, then we must be prepared also to accept the fundamental condition that makes relativity theory more than a mere babel of voices. The relativity principle in psychology may be stated in some such fashion as this: *The laws of psychology must be invariant under transformation from one system to another.* If such a condition is not accepted, then clearly chaos results. But here we are struck by another idea. If we accept the principle of invariance for psychological systems, does it not follow that these invariants must be expressed in some language, i.e., some frame of reference which, in a sense, must transcend all these other relative ones? We are then confronted with two problems different in nature: What are the invariants and what do they represent? The answer to this may

[1] Read to Southern Society for Philosophy and Psychology, Roanoke, Virginia, March 23, 1951.

lead us to a more precise definition of the subject matter of psychology than has hitherto been given. But for this paper, the more interesting question is this: Is this fact, the principle of invariance, itself meaningful in a frame of reference only? If it is, we are confronted by an infinite regress of frames of reference and we are involved either in a difficulty analogous to the logical problem of the theory of types in logic or to a kind of Gödel theorem. In both cases, we would be led to sterility in the controversy over frames of reference or to some form of unanswerable questions about frames of reference. The practical importance of these considerations may be indicated in the following manner. A good deal of work has been done to show the effect of frames of reference. So Asch (1) and others showed that judgments of traits like honesty, intelligence, etc. were a function of the individual's frame of reference. Even more vividly is this indicated in the study by Stagner (7) on Fascist attitudes in which a relation was established between economic status and pro-Fascist attitudes. Also Sherif (5) has established that the frame of reference tends to be established by the group. The implications of these experiments for the construction of theories and for the selection of frames of reference should be obvious. If the principles disclosed in these studies can be generalized to be applicable to scientific judgments, scientific opinions and scientific facts, then the implied relativism would make of the scientific enterprise merely a kind of "class-science" in a vicious form. No meaning could be given to the expression "objectivity of science" and no controversy that existed between frames of reference could be decided. Yet the question presents itself: In what frame of reference is the assertion of the relativism of frames of reference meaningful? In other words, it appears to me that the very assertion of the relativism of frames of reference involves the tacit assumption of some over-all frame of reference. From the point of view of theoretical psychology, it may be asked, when Sherif, e.g., points out the effect of frames of reference on perception, in what frame of reference is this study embedded, i.e., in what frame of reference are his perceptions of these effects meaningful? In the light of what I have said, the possibility arises that all facts about the effects of frames of reference on perception are meaningless in other frames of reference. If this possibility is to be avoided, then facts about frames of reference ought to be translatable into facts in other *acceptable* frames of reference.

In order to clarify my point, let me take two statements as examples:

1. What is perceived is determined by cultural conditions.
2. Neurotic behavior is the response of the individual to threats to his phenomenal self.

Although not direct quotations, it is apparent that the first might have been taken from Sherif's monograph, and the second from Snygg and Combs.

Each of these statements is constructed within a given frame of reference, using, therefore, the basic concepts and definitions of the respective frames. Since the second sentence contains the concept phenomenal self, which we shall assume is not present in Sherif's frame of reference, the second sentence is either meaningless in Sherif's frame of reference or it can be made meaningful by defining "phenomenal self" in Sherif's terms, if that is possible. Suppose it were not possible to define the concept in Sherif's terms? Snygg and Combs clearly mean to assert the *existence* of something which they call by this term. Therefore, if it is not possible to define this term in Sherif's

frame of reference, we would seem to be left with the following alternatives: (*a*) Sherif's frame of reference is too narrow since there exists an X which cannot be subsumed into it; (*b*) Snygg and Combs' frame of reference is too wide since it includes a term which cannot be embedded in Sherif's frame of reference. The question depends on the existence of a referent for a term. If the referent can be established as existing, then Snygg and Combs' frame of reference would appear to be preferable in this respect to that of Sherif's, because it includes a concept or term to denote that referent while Sherif's does not.

But this outcome immediately raises another question. Consider the statement: There exists a phenomenal self.

If we accept the notion that facts are such only within a frame of reference, and that statements are meaningful and true in a similar fashion, then the assertion of the existence of the phenomenal self is meaningful *only within the frame of reference of Snygg and Combs,* and meaningless in that of Sherif's *unless it can be translated into a statement in Sherif's frame of reference.* Therefore, the statement "a phenomenal self exists" cannot be used to refute Sherif's system. Furthermore, in Snygg and Combs' system, the statement is true *by definition* since the concept "phenomenal self" is so defined as to refer to certain already selected phenomena. Hence the assertion of the existence of the phenomenal self can never be rejected as long as one stays within the given system. If this is the last word, then clearly frames of reference are simply projections of the personality of the particular scientist—but even such an assertion is meaningful only in a frame of reference and the whole scientific task dies. The only way out is to recognize that an existence statement must have a reference beyond the bounds of the system in which it occurs and its verification is, in a sense, independent of any particular frame of reference. Failure to be able to translate an existence statement from one frame of reference to another would appear to leave only the following alternatives: Either the object asserted to exist does not exist, or the system in which its existence cannot be asserted is inadequate. In specific terms: If in Snygg and Combs we assert the existence of the phenomenal self, and this cannot be translated into an existence statement in Sherif's system, then either the phenomenal self does not exist, or Sherif's system is inadequate. In passing, we note that the failure to show the phenomenal self to exist does not mean a necessary rejection of the concept since clearly it may be a useful fiction or an intervening variable or a construct.

This part of our discussion leads, it seems to me, to a second area of invariance. Not merely must the laws of psychology be invariant under transformation from one system to another, but (*a*) *existence statements must transform into existence statements,* and (*b*) *if an existence statement is true in one system or frame of reference, it must be true in any other.* This second principle has rather startling implications. It means, for example, that a statement, e.g., in Freudian psychology such as: "the id exists," must transform into an existence statement in, e.g., behavioristic terms. That is why the book on *Personality and Psychotherapy* by Dollard and Miller (2) becomes important. If the statement "the id exists" is true in Freudian psychology, its transform must likewise be true in a behavioristic frame of reference.

These considerations lead to some elementary considerations. If a statement in a given system (call it S_1) must be transformable into a statement (S_2) in another system in such fashion that if

S_1 is true (or false), S_2 must be likewise, then this is possible if S_1 and S_2 are synonymous. But synonymity means equivalence of meaning or identity of referent. This implies, it appears to me, that the meaning of a statement must be clearly separated from its expression and is, therefore, independent of the frame of reference in which it is expressed. In order to take care of those cases where statements are about intervening variables or constructs, we have separated "meaning" and "referent." No one would, probably, confuse the referent with its description or with a statement about it.

With these thoughts in mind, we can say that by "phenomenon" we shall refer to what is meant or referred to. This would, then, lead us to say that science is concerned not with facts but with *phenomena*. Now, if S_1 is a statement in a given frame of reference about a given or even a presumed phenomenon, then it must be transformable into S_2, a statement in another frame of reference, in such a way that if S_1 is true (or false), then S_2 must also be true (or false). In particular, if S_1 is a statement of the form (*a*) "there exists an X, such that S_1 (X)," then this must be transformable into a statement of the form (*b*) "there exists an X, such that S_2 (X)." If (*a*) is true, (*b*) must be, and if (*a*) is false, (*b*) must also be. This means that we can now distinguish between true statements and false ones and use the word "facts" to refer to the former. Both "facts" and "non-facts" can occur in any system on the basis of these distinctions, and the task of the investigator is to be able to write down as many facts as possible. Also, a fact in one system must be transformable into a fact in another or else the system into which it cannot be stated must be rejected as inadequate at best and wrong at worst. ***The invariants for both systems are the phenomena.***

It is necessary, therefore, to differentiate two aspects of any frame of reference: (*a*) its formal structure, and (*b*) its material signification. I shall not consider its formal structure because by now the form of a scientific system has been discussed at length and is relatively familiar to most investigators. Also, as is well known, the interpretation of the formal structure consists in the establishment of a set of rules of correspondence for fixing the meaning of the set of symbols. A system S_1 and another S_2 may have the same structure and may use the same or different symbols. They may use the same symbols to denote different phenomena, or different symbols to denote the same phenomena. When different symbols are used to denote the same phenomena, the task of transformation is easy. It amounts to establishing a dictionary of one frame of reference in terms of the other and S_1 transforms easily into S_2. Where the same symbols are used in S_1 and S_2 to denote different phenomena, then the relativism may be overcome by the simple expedient of constructing a system S_3, using a totally new set of symbols and establishing a dictionary for it in terms of S_1 and also another in terms of S_2. The statements in S_1 will translate into statements in S_3 which are then translatable into statements in S_2. S_1 and S_2 can be compared through S_3. Notice that this depends on being able to "know" S_1 and S_2, i.e., to be able to understand their meanings. Where different sets of symbols are used for different sets of phenomena, we may have two complementary systems. In every case the relativism must be transcended by reference to the phenomena.

There is one final set of considerations that needs to be recognized. A system S_1 may use, as a basic set of symbols, those referring to a given set of phenomena which are derivative in S_2. All

that has been said remains unaltered. The statements must be transformable within the systems. However, statements *about* the systems will differ, but with this we are not concerned. Which set of phenomena are to be symbolized as basic, depends on two considerations which are often confused: (*a*) metalinguistic ones, e.g., simplicity of system structure, fruitfulness, etc., and (*b*) material ones, e.g., whether as a matter of fact neuroses are reducible to repressions.

In conclusion, we point out that the important thing is the phenomena which give rise to facts, or which are what make statements into facts. It is the number of facts which one can derive that makes a system fruitful and applicable and not the number of statements. And while frames of reference are partially relative, the acceptance of a frame of reference is a function of its adherence to the principle of the relativity of systems—a principle which introduces an element of invariance and nonrelativity.

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LEARNING AND THE SCIENTIFIC ENTERPRISE

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I

Whoever has given thought to the problems inherent in current views of learning phenomena might well wonder whether it is appropriate, at the present time, possibly to confound the issues by drawing attention to the fact that the scientific enterprise itself is a learning enterprise. In a sense, this point is banal. In another sense, it is not; for it is a point that has been largely ignored, and a point which has been systematically banished (2) by some theorists.

Fairly recently Skinner (7) has raised the question, "Are theories of learning necessary?" and has answered it in the negative. He has indicated that we can do better and faster without the types of learning theories which are currently available. In effect, what Skinner has done is to raise the controversy between empiricism and rationalism. But in doing so, Skinner also advances, by implication, a view concerning the nature of human learning. In effect, what Skinner is saying is that the attitude of empiricism is more conducive to efficient learning than is the attitude of rationalism. He makes an assertion about the role of attitude in human learning. This he does by advocating that the scientist shall concern himself primarily with the collection of data rather than the construction of elaborate theoretical systems. To quote him directly:

> An adequate impetus is supplied by the inclination to obtain data showing orderly changes characteristic of the learning process. An acceptable scientific program is to collect data of this sort and to relate them to manipulable variables, selected for study through a common sense exploration of the field (7, p. 215).

In advocating one "scientific" approach rather than another, one must necessarily be presupposing an hypothesis concerning the nature of human learning. The scientific enterprise is a learning enterprise. Thus any point of view which asserts what is good scientific method, presupposes a theory of human learning. The thesis of this essay is that if we were carefully to examine our views concerning the nature of the scientific enterprise, we might be in an excellent position to advance our understanding of the processes of human learning. In the same way that we were able to advance our understanding about the nature of human personality by the study of extremes, so might we be able to advance our understanding of the learning process by the study of an extreme in human learning, namely, the learning of the scientist.

There is a point of view in the field of psychology which sets up a "methodological" distinction between the scientist and the subject (2). "Methodologically" it differentiates between the man in his functioning as a scientist and in his functioning as a human being. This distinction is both unfortunate and unreal. Its maintenance closes off from our view one of the richest sources of information about human learning that we have. The distinction makes it impossible, in principle, if not in practice, to study the learning of the scientist in his learning operations. In advocating this distinction Bergmann and Spence (2) write:

> In the schema outlined by the scientific empiricist the experiences of the observing scientist do indeed have a privileged, even unique, position. If pressed too far and without the necessary epistemological so-

phistication, this account of the scientist's position can very easily lead to a metaphysical thesis of the solipsistic type (2, p. 3).

The nature of this "epistemological sophistication" is given to us by these writers as follows:

> . . . the empiristic scientist should realize that his behavior, symbolic or otherwise, does not lie on the same methodological level as the responses of his subjects . . . (2, p. 4).

Whatever "methodological" distinctions we may attempt to draw, we cannot escape from the brute fact that the scientist's "behavior, symbolic or otherwise" is behavior; and is, more particularly, learning behavior. We are hardly convinced that the acceptance of what the scientist does as behavior will lead us to solipsism, with the exception of one aspect of the solipsist position. The aspect of solipsism to which we have reference is its emphasis on the creative character of learning, a feature indicated by Tolman (10), who is hardly a solipsist. Indeed, the methodological distinction *preserves* rather than justifies a "privileged . . . unique position" for the experiences of the scientist. Thereby it grants creativity to the scientist, but denies it to the subject.

Not only does this distinction between scientist and subject cut off the opportunity of the examination of the scientific enterprise as an example of learning, but also tends to lead us into error in our investigations. In order to demonstrate this latter point, let us examine the notion that the stimulus is an independent variable. We take the notion of the stimulus as an example to point out one type of error that the "methodological" distinction can foster; and also we offer the analysis of the stimulus notion as an example of the type of analysis of the scientific enterprise which is being advocated.

II

The stimulus as an independent variable is a notion that is quite taken for granted by the "scientific empiricist." Spence (8) characterizes stimulus variables as follows:

> S-variables: measurements of physical and social environmental factors and conditions (present and past) under which the responses of organisms occur. These are sometimes referred to as the independent, manipulable variables (8, p. 48 f.).

The questions of whether the word "variable" applies, or the word "independent" applies, are not generally raised. Certainly the recent work on the effect of personality and attitude on perception can well lead us to ask whether we really know what we mean by a stimulus. A recognition of the difficulty inherent in this matter of perception is manifested in the effort to reconcile the lack of independence of the stimulus with Hullian theory (3).

What do we mean by a variable? Let us consider two items, the status of which as variables would hardly be challenged, say, length and color. By virtue of what may these be considered to be variables? First, that they vary, i.e., that there are two or more lengths, and that there are two or more colors. Second, that the designated categories of the variables shall be mutually exclusive, i.e., if something be of one length, it not be of another length, and if something be of one color, it not be of another color. That which distinguishes one variable from another is that the criterion of mutual exclusiveness does not apply, i.e., if something be of one color it may be of any length, and if something be of one length, it may be of any color. Thus we arrive at a definition of a variable:

A variable is a set of two or more categories such that, if any object or event be a member of one of those cate-

gories, it may not be a member of any other of those categories.

Let us consider some of the implications of this definition for the notion of a stimulus variable. Let us avoid getting into the metaphysical question by simply saying that the real world is what it is, without any further elaboration. However, whatever the nature of the real world may be, and without in any way denying the existence of the real world, the *constitution* [1] of the variable *as a variable* is the work, or invention, or creation, of the scientist. Variables, whatever they may be *in re*, do not exist there *as variables*. For, variables are, by definition, sets of categories; and *categories are the result of someone's delineation, abstraction, and identification.*

By virtue of these considerations, if it were not so tedious, it would be more appropriate to talk of variables-as-constituted-by-the-scientist, rather than simply of variables. The refinement that is introduced by this distinction is less important in the physical sciences, since, in the latter, it would be rather meaningless to talk of variables-as-constituted-by-the-subject. Even in the physical sciences, following the notions of Bridgman (4), the distinction is appropriate. However, this distinction is extremely important in the psychology of learning, for here we have the constitution of variables both by the experimenter and by the subject.

To clarify this point, let us consider a more or less typical experimental situation in the psychology of learning. Consider a Skinner box, with a lever, the depression of which by the animal results in the delivery of a food pellet. The animal is placed in the box. Is there any constitution of the stimulus variable on the part of the animal? Not at the outset of the experiment. However, the lever is constituted at the outset of the experiment as a stimulus variable by the experimenter. (The categories of the variable in this case are presence or absence of the lever.) It is *delineated* from the remainder of the environment, the property of leverness has been *abstracted*, and it is *identified* as a lever, the depression of which leads to the delivery of a food pellet, *by the experimenter* at the outset of the experiment.

As "learning" takes place or proceeds, the animal comes to depress the lever with greater and greater regularity. The animal, it may be said, comes to learn *what's what*. We prefer "what's what" to the Tolmanian "what leads to what" since the former, to us, seems to include the delineation and the abstraction more than the latter does. The animal delineates the lever from the remainder of the environment, avails itself of its leverness, and identifies it as leading to food. Essentially, what takes place in this type of experimental situation is that the animal comes, after the learning process, to constitute the stimulus variable in much the same way as the experimenter did at the outset of the experiment.

The usual experimental learning situation is a teaching situation, more particularly, than a learning situation. The *success* of the learning is indicated by the degree to which the animal seems to have constituted the stimulus variable as the experimenter has constituted the stimulus variable. The latter is exactly what is meant by the *learning criterion* in most experiments in the psychology of learning.

The "methodological" distinction between scientist and subject prevents us from seeing that the *psychological processes of the experimenter are an integral*

[1] We borrow this word from Dewey's *The reflex arc concept in psychology* (5). Much of what is being said here is based on our interpretation of some of the notions in Dewey's paper.

part of the nature of the experimental situation. Furthermore it leads us into the error of generalizing from such experimental situations to learning in general, when actually it is the special case of learning which we might call teaching or communication. "Methodologically" it removes the experimenter from the "data" when actually his psychological processes are "data" too; and necessary data for the understanding of the nature of the experimental situation.

III

We have concerned ourselves with the *variable* aspect of the stimulus variable. Let us now turn our attention to the question of the *independence* of the stimulus variable. What is generally meant by the assertion of the independence of the stimulus is that it is independent of the response. The paradigm is that the response is dependent on the stimulus, but the stimulus is independent of the response. The formulation of this paradigm is

$$R = f(S).$$

But even here, the matter is begged. For, in the explanation of what a stimulus variable is, the expression "under which the responses of organisms occur" (8) is used. The stimulus does not exist as a stimulus except by virtue of the responses of the organism. The delineation, abstraction, and identification of the stimulus *is the response.*

The only kind of independence that the stimulus variable can have, is a spurious one. The delineation, abstraction, and identification of the stimulus variable *by the experimenter* is independent of the delineation, abstraction, and identification of the stimulus variable *by the subject.* But in the subject itself, there is no such independence.

The operationist argument to the effect that our knowledge is contingent upon the operations that we engage in in obtaining this knowledge can well be leveled against the conception that the stimulus variable is independent of the response. For we cannot even identify the stimulus properly without taking account of the responses of the organism. If this is true, we can hardly talk of the independence of the stimulus variable.

Again, we might point out that the fallacy of considering the stimulus variable as independent may be viewed as stemming from the "methodological" distinction between scientist and subject. It can be said that, by virtue of not taking account of the psychological processes of the investigator, the investigator is led into a position where he "projects" his prior constitution of the stimulus into the organism that is the subject in the experiment.

IV

The point which has been made above, that it is inappropriate to talk of the independent stimulus variable, has, interestingly enough, been made in current discussions of the scientific method by some of the same persons who talk of the independent stimulus variable. The point is one of the essential ones of operationism. It has, however, not been brought to bear as an insight concerning learning. Thus, for example, Bridgman (4) has given priority, not to the stimulus, but to the response. Basic to scientific learning is not the stimulus, but the *operations of the scientist.* Similarly, Stevens (9), accepting the priority of the operation, goes on further to specify the nature of the operation as being *discrimination,* a response. Bergmann and Spence (1), aside from their particular views on the psychology of learning proper, have emphasized the response feature of the scientist's learning, in their emphasis on theory construction and theory creation. In spite of their avowed point of view concerning the

distinction between scientist and subject, they still permit themselves to say:

> Historically and *psychologically*, then, the *creation* of helpful concepts is a very essential part of a scientific achievement (1, p. 3). (our italics).

and:

> Actually much of what is usually called theorizing in empirical science consists . . . in the *creation* of these organizing empirical constructs . . . (1, p. 6) (our italics).

In these writings we find not only a stress upon the response priority in the learning situation, but also the creative aspects which are involved in learning. We have here a stress upon a feature of human learning which is rarely mentioned in the works on the psychology of learning proper. It is of more than parenthetical interest that Hull's *Principles of Behavior* (6), an effort in human learning, is certainly worthy of the adjective "creative."

V

There is certainly much more to be learned about the nature of human learning by the consideration of the scientific enterprise as an instance of human learning. Here we have made no more than the attempt to suggest where this notion might lead, and to outline a program in broad brush strokes. In the scientific enterprise we have an example of learning *de novo* as contrasted with learning by being taught. By the study of it we might learn a great deal concerning the nature of learning.

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VALUES, WORD FREQUENCIES, AND PERCEPTION

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In a recent article Solomon and Howes (11) have made two points about the research by Postman, Bruner, and McGinnies (7) supposedly demonstrating a relationship between value and perception:

1. Since duration thereshold (i.e., duration of tachistoscopic exposure necessary for recognition to occur) is related to word frequency,[1] the relation of duration threshold to value rank on the Allport-Vernon test (1) can be accounted for by assuming that "for words representing a field of interest, valuation of that interest is associated with a departure from the mean frequency of use in the general population" (11, appendix).

2. The Allport-Vernon test itself can be considered a measure of the frequency with which the subject uses certain words—and nothing more than that; that is, there is no need to postulate or infer any vague entities like "values" in order to account for value ranks obtained when the test is administered to a subject. The authors make this point quite explicit by postulating two hypothetical populations A and B which would obtain different value ranks on the test even though "the only difference between the two populations is the frequency with which they use the two sets of words" (11, p. 265).

In replying to Solomon and Howes, Postman and Schneider (8) merely gather additional data demonstrating the relation between value rank (as measured by the Allport-Vernon test) and perceptual-cognitive functions (visual recognition and recall). They obtain significant differences between duration thresholds for both relatively high- and relatively low-frequency words (in the general population) as a function of value rank. Though they disagree with Solomon and Howes with regard to the question of hypothetical cognitive and motivational processes versus stimulus response correlations and response probabilities without such hypothetical constructs, the fact remains that their results can be predicted on the basis of point 1 above plus a slight additional assumption about the relation of word frequency to recall.

We do not wish to take issue with point 1 made by Solomon and Howes; it seems plausible enough and should be taken into account by anyone doing research on the relation of motivational processes or cognitive structures to duration thresholds, when words are used as stimuli. This point should probably be modified, however, if, as it seems to us, the Allport-Vernon test confounds to some extent two psychological dimensions which can be separated, namely *interest* and *value*. An individual can be interested in a given area even though he has a strong disagreement with individuals or institutions operating in that area. For example, a militant atheist may be very interested in religion though harboring little value for religious beliefs or experience. Because of the way in which the Allport-Vernon test is constructed and scored, it seems to us that interest and value are confounded, though no doubt these two variables are

[1] This relation was found by Howes and Solomon (4) to be logarithmic, i.e., $t = a \log f + b$, in which t is duration threshold, f is word frequency, and a and b are constants. The scatterplots which the authors present, however, seem to indicate a nonlinear relation between t and $\log f$.

correlated to some extent. It is reasonable to believe that whatever correlation exists between word frequency and value rank on the test can be accounted for more by the interest variable than the value variable; at any rate this hypothesis could be tested. Of course our formulation will not be acceptable to Solomon and Howes; if they wish to make this distinction they will have to redefine "interest" and "value" in accordance with their own theoretical orientation (see below).

With regard to point 2 we wish to point out the following: (*a*) The analysis by Solomon and Howes of what happens when a subject takes the Allport-Vernon test is somewhat unsound; (*b*) the Allport-Vernon test is inaccurately described by Solomon and Howes, even with respect to the mechanical features of taking and scoring the test.

Let us first describe roughly the two interpretations given by Solomon and Howes of what happens when a subject takes the Allport-Vernon test. As the reader may have the impression that we are misrepresenting these interpretations, we refer him to the careful exposition by Solomon and Howes on pages 265 and 266 under the heading "The Allport-Vernon test as a corollary of word frequency." First of all, Solomon and Howes assert that the test can be considered as a "set of visual choice discriminations between alternative groups of words, the choice being presented as a series of paired comparisons. Each comparison is between words representing two different interest areas. For example, consider item 4 of the test: 'If you were a university professor and had the necessary ability, would you prefer to teach: (*a*) poetry; (*b*) chemistry and physics?'" Solomon and Howes assert that if a subject were to indicate his choice by responding vocally, then, if he had a lower duration threshold for "poetry" than for "chemistry and physics," he would have a tendency to make the response "poetry," because "on this test the operations performed are identical to those defining duration threshold" (11, p. 265). They then assume "the response of checking a number that corresponds to a printed word does not differ fundamentally from the response of emitting the vocal sounds" (cf. Weiss (14, pp. 301–304)). Since duration threshold has been shown to be related to word frequency, the Allport-Vernon test is thus reduced to a corollary of word frequency.

It seems to us that the operations performed in giving and taking the Allport-Vernon test are not identical with those defining duration threshold. If the subject were asked to read the first part of the item, then glance at the remainder and check the first word that he sees, then perhaps the operations would be highly similar, though such methodological controls as fixation point, etc., would be absent. Furthermore, when Solomon and Howes quote their own experiment, "we know from (4) that, following any given amount of exposure to the printed alternatives, the word with the higher frequency of use will tend to occur more often," they overlook the fact that in their experiment (4) this relationship holds over only a very short range of time exposures, and that unless they can show that the time of exposure of the alternatives on the Allport-Vernon test is within this range, they are extrapolating their results in a most extraordinary way.

The second interpretation of what happens when a subject takes the Allport-Vernon test runs somewhat as follows: the answer indicated by the subject by "circling" an answer is determined by the strength of association between the word (visual stimulus) and the response (circling); "the printed words *poetry* and *chemistry or physics* are the discriminative stimuli and the

circling of an answer is the overt response in this discrimination. In conventional association interpretation this response is assumed to be mediated by the covert response of one of the alternative word-pairs, and the appropriate circling response will depend upon which mediating response is more strongly associated with its visual stimulation" (11, p. 266). If this interpretation is correct, then perhaps the first part of the item can be dispensed with. We are willing to predict that dispensing with the first part of the item will give somewhat different results.

Our second point may make the preceding discussion seem rather academic. The subject does not indicate his answer on the Allport-Vernon test by checking or circling a number, letter, or word. We quote from the directions of the test (1) and present the first item:

A number of controversial statements or questions with two alternative answers are given below. Indicate your personal preferences by writing the appropriate figures in the right-hand columns, as indicated:

	(*a*)	(*b*)
If you agree with alternative (*a*) and disagree with (*b*), write 3 in the first column and 0 in the second column, thus	3	0
If you agree with (*b*); disagree with (*a*), write	0	3
If you have a slight preference for (*a*) over (*b*), write	2	1
If you have a slight preference for (*b*) over (*a*), write	1	2
1. The main object of scientific research should be the discovery of pure truth rather than its practical applications. (*a*) Yes; (*b*) No.	(*a*) —	(*b*) —

Items 2 and 3 are also of the Yes-No variety, as are 12 items in all of the 30 items in the first part of the test.

Part II of the test has somewhat different directions from those of Part I:

Each of the following situations or questions is followed by four possible attitudes or answers. Arrange these answers in the order of your personal preference from first to fourth by writing, in the left-hand margin:

..1.. beside the answer that appeals to you most,
..2.. beside the answer which is next most important to you,
..3.. beside the next, and
..4.. beside the answer that least represents your interest or preference.

1. Do you think that a good government should aim chiefly at
..... *a.* more aid for the poor, sick, and old,
..... *b.* the development of manufacturing and trade,
..... *c.* introducing more ethical principles into its policies and diplomacy,
..... *d.* establishing a position of prestige and respect among nations.

It would appear, from a quick inspection of the Allport-Vernon test directions and items, that the choice involves something more than a simple paired comparison, not to mention the fact that a significant part of one's value score seems in Solomon's and Howes's terms to be dependent upon one's fortuitous experiences with the words "Yes" and "No."

However, we have constructed a modified Allport-Vernon test in which a circling response is made to test Solomon and Howe's hypothesis about the test as they conceive it.

We should now like to make a few general remarks about the theoretical orientation of Solomon and Howes. These authors attempt to rid psychology of vague mentalistic terms like "perception" (as ordinarily used) by defining "seeing," "perception," and other vague mentalistic terms operationally by care-

ful specification of stimulus and response properties and their publicly observable relationships. This orientation is shared by some other psychologists (2, 3, 5, 6, 10, 12, 13, 14). Solomon and Howes speak of "optical situations" and "visual choice discriminations" (e.g. as in the Allport-Vernon testing situation), "linguistic responses," "circling responses," etc., implying that a great gain in scientific advance in psychology would result were other psychologists to do likewise. The operationist-nonoperationist, private-public controversy is one which we certainly do not wish to attempt to discuss in detail in this paper. The argument is in part a semantic one, because any psychologist, whether he *defines* "perception," etc. operationally or not, has to develop operational *indices* of what he wants to study whenever he uses other individuals as subjects. We might call the first an "Operationist" and the second an "operationist." Many psychologists who are operationists are not Operationists. The controversy is then in part a controversy about whether observables are to be used as indices of something else or whether the observables are to be the defined subject matter of investigation. This controversy is not entirely semantic, however, because it is in part a controversy about (*a*) whether phenomenology is an acceptable method, (*b*) what terms psychologist should use in their "thinking" so as to advance psychology as a science.

The authors believe that psychologists can sometimes obtain valid knowledge (not mere *hints* about how to proceed with other subjects) by observing contrast effects, after-images, apparent movement, figural after-effects, etc. We also believe that it sometimes pays a psychologist when talking about certain phenomena, e.g., after-images, to think in terms of vague, mentalistic events, like after-images, rather than stimulus-response correlations (cf. Pratt, 9). Further, we challenge Operationists to prove by an examination of the history of science or any other method that "Operational" thinking is in all cases—or even in most cases—the most advantageous way of thinking. We do not believe, moreover, that certain Operationists have freed themselves from mentalistic ways of thinking. It seems to us that if Solomon and Howes were consistent in thinking of the Allport-Vernon test in terms of "optical operations" and "circling responses," they would, being good psychologists, have been careful to control such conditions as fixation point, time of exposure, position of the pencil and arm on the table, etc., and perhaps they would have carefully distinguished between checking and circling as different motor responses.

It would be possible to make a direct test of Solomon and Howes's frequency hypothesis by altering the Allport-Vernon test in several ways. Two methods that immediately suggest themselves would be to put all the statements in a negative way by the use of "not," "rarely," "hardly ever," or "never." Presumably, then, the subject would have to resist his "circling responses" to the high-frequency valued words and make the response contrary to frequency. We have revised the test in such a way that the alternatives are all in very infrequent synonyms except those scoring in one value area which are phrased in very frequent words. Six such forms of the test have been constructed, each having one of the value areas favored frequencywise. Presumably the subject would be duped into the high-frequency words and thus the value area so designated by the experimenter.

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PRELIMINARY SUGGESTIONS AS TO A FORMALIZATION OF EXPECTANCY THEORY[1]

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In the present paper we shall try to indicate one direction in which a rigorization of expectancy theory might move, concentrating wholly upon what we believe are the major constructs of such a theory. In a previous paper (**9**), we discussed briefly the question, "How may an expectancy theory of learning be identified as such?" We took the position that several features in the thinking of expectancy theorists (e.g., Tolman) are not logically entailed by the admission of an expectancy construct, and we suggested that one (and perhaps *the*) crucial differentiator between an expectancy and a nonexpectancy theory is the form of the acquisition postulates. One particular type of acquisition postulate, providing for the strengthening of the basic learning element in a special way, we called the "Inference Postulate," listed as Number 4 below.

While the presence of such a postulate generates many properties in the theory which are absent without it, strictly speaking it is always the entire *system* of postulates which does the generating. The inference postulate, for example, cannot lead to the desired deductions concerning latent learning unless the role of the expectancy construct in activating behavior is also indicated (our postulate 12). This latter indication in turn requires some subset of postulates defining motive-incentive constructs (as our postulates 6, 7, 9, 10, 11), and so it goes. Ideally, the implicit definition of these constructs would be given by the entire, "complete" postulate system; their empirical meaning would be, so to say, exhibited, *shown forth* by the interdigitation of the propositions with one another and with the behavioral theorems they jointly entail.

In what follows we shall present an *incomplete*, *tentative*, and certainly *nonsufficient* set of propositions which, however, begin to define at least one important kind of expectancy theory. This formulation has a major affinity with that of Tolman because it contains an inference postulate, and (what in turn makes *this* possible) because its fundamental cognitive unit, the "expectancy" (S_1RS_2) is designated by three notational elements rather than the two which specify Hull's ${}_sH_R$. In this set, then, the basic construct of learning specifies not only the S_1 in the presence of which the organism emits R, but in addition it specifies what is expected when R follows S_1.

On the other hand, this formulation differs from Tolman's in several respects; of these differences we currently consider only one to be clearly fundamental. The fundamental difference is our inclusion of an *R reference* in the notation specifying the basic cognitive unit, the "expectancy." In this sense the construct lies somewhere between the constructs of Tolman and those of Hull, since it includes the S_2 reference (unlike Hull)

[1] This paper grew out of discussions at the Social Science Research Council-subsidized Dartmouth Conference on Learning Theory (1950). Although they are in no sense to be considered as endorsing our formulations, the other members of the Conference—Drs. W. K. Estes, C. G. Mueller, S. Koch, W. N. Schoenfeld, and W. S. Verplanck—contributed both stimulation and criticism.

but also the *R* reference (unlike Tolman). It is true that in some of his discussions (e.g., **14**, pp. 10–12, 82) Tolman seems to include the response reference in his idea of an expectancy; but his more recent emphasis on perception and the "map" metaphor have deflected attention from the *R* term. This sort of emphasis leads him to have some difficulty in *getting to behavior*, which is expressed in Guthrie's well-known gibe about Tolman's leaving the rat "buried in thought." We try to solve the problem by including an *R* reference as part of the expectancy construct from the beginning. Let us emphasize, however, that this *must not be understood to mean that an expectancy IS a "response"* if the word response means an effector-event-class; nor does it mean that the arousal of an expectancy is an effector-event, albeit a damped or attenuated one.

The facets of Tolman's approach which we do not see as crucial or differentiating were merely listed in our previous paper (**9**, p. 230). Professor Tolman informs us (personal communication) that he agrees with us about four of them, but he feels that two of them, the "Gestalt-configural stress" and the "specification of reaction-class by reference to position, direction, or locomotion (rather than by effector properties)," are integral features of his view. Space does not permit us the detailed consideration of these two points which would be necessary to do them justice, but this whole question will be treated in detail in a subsequent publication.

We cannot stress too strongly that the following is *not* offered as a full-fledged "postulate set." The postulates represent our first attempts to nail down the expectancy view. They are obviously incomplete even if the theorem system required were a "miniature" one, e. g., confined to rat behavior in the Skinner box. The great mass of data regarding temporal effects, for instance, is left quite untreated. The notion of "similarity" between two stimulus configurations is left unclarified; there is no attempt to treat the entire area of facts and concepts usually called "inhibition," or the allied phenomena of non-extinctive work decrement. The postulate of need strength is not clearly supported by current evidence on alimentary drives and there is no good evidence that for other "higher-order" need variables it should be expected to hold at all. There are no postulates regarding the very important question of interaction among expectancies to yield some compromise *R* strength. We have not felt it propitious even to make guesses about the exact *form* of functions, at this point. But anyone who wishes to substitute, e.g., "simple positive growth function" wherever we have written "increasing decelerated function" can easily do so.

Since one of the constantly reiterated complaints against expectancy theorists for almost twenty years has been the extreme sketchiness of their formulations, we perhaps do not need to apologize for the present effort if it succeeds in reducing this sketchiness even slightly.

Furthermore, the present article aims merely to *present* the incomplete set and to *illustrate* its workings by deriving some semiquantitative consequences. No attempt is made to *defend* our particular decisions as to formulation, or to develop the thinking behind each. This more extended treatment will appear in a subsequent publication.

Pending detailed treatment, we shall merely say that in what follows the term *stimulus* may include a physical situation of any describable complexity or patterning. That is,

if "triangularity" is the property needed to yield a response-inferred stimulus equivalence, the relational features between three points are, of course, included in the specification of S. It is not suggested that there are no unsolved problems here; but since, like Skinner, we are unconvinced of the definitive role of "configural properties" as marking out kinds of learning theories, we have ignored these issues for present purposes. In the same way, we mean by *response* a class of effector activities which produce environmental effects within a specified range of values, e.g., "turning left," "pressing lever," and the like. (Cf. **5**, pp. 95–96; **12**, pp. 33–43.) Again, this summary treatment springs from an acute awareness of the terrible difficulties in the response concept rather than a naive belief that they have been solved.

The postulates are as follows:

1. *Mnemonization*: The occurrence of the sequence $S_1 \rightarrow R_1 \rightarrow S_2$ (the adjacent members being in close temporal contiguity) results in an increment in the strength of an expectancy $(S_1R_1S_2)$. The strength increases as a decelerated function of the number of occurrences of the sequence. The growth rate is an increasing function of the absolute value of the valence of S_2. If the termination by S_2 of the sequence $(S_1 \rightarrow R_1)$ is random with respect to nondefining properties of S_1, the asymptote of strength is $\leq$ the relative frequency P of S_2 following $S_1 \rightarrow R_1$ (i.e., a pure number). How far this asymptote is below P is a decelerated function of the delay between the inception of R_1 and the occurrence of S_2.

2. *Extinction*: The occurrence of a sequence $(S_1 \rightarrow R_1)$ if not terminated by S_2 produces a decrement in the expectancy if the objective S_2-probability has been 1.00, and the magnitude of this decrement is an increasing function of the valence of S_2 and the current strength of $(S_1R_1S_2)$. Such a failure of S_2 when P has been $\doteq 1$ is a *disconfirmation* provided $(S_1R_1S_2)$ was nonzero. If the objective probability P shifts to a lower P', and remains stable there, the expectancy strength will approach some value $\leq P'$ asymptotically.

3. *Primary Generalization*: When an expectancy $(S_1R_1S_2)$ is raised to some strength, expectancies sharing the R and S_2 terms and resembling it on the elicitor side will receive some strength, this generalized strength being a function of the similarity of their elicitors to S_1. The same is true of extinction of $(S_1R_1S_2)$.

4. *Inference*: The occurrence of a temporal contiguity S_2S^* when $(S_1R_1S_2)$ has nonzero strength, produces an increment in the strength of a new expectancy $(S_1R_1S^*)$. The induced strength increases as a decelerated function of the number of such contiguities. The asymptote is the strength of $(S_1R_1S_2)$ and the growth rate is an increasing decelerated function of the absolute valence of S^*. The presentation of S_2 without S^* weakens such an induced expectancy $(S_1R_1S^*)$. The decrement is greater if the failure of S^* occurs at the termination of the sequence $S_1 \rightarrow R_1 \rightarrow S_2$ than if it occurs by presentation of S_2 not following an occurrence of the sequence.

5. *Generalized Inference*: The occurrence of a temporal contiguity S_2S^* produces an increment in the strength of an expectancy $(S_1R_1S^*)$ provided that an expectancy $(S_1R_1S'_2)$ was at some strength and the expectandum S'_2 is similar to S_2. The induced strength increases as a decelerated function of the number of such contiguities. The asymptote is a func-

tion of the strength of $(S_1R_1S'_2)$ and the difference between S_2 and S'_2. The growth rate to this asymptote is an increasing decelerated function of the absolute valence of S^*.

6. *Secondary Cathexis:* The contiguity of S_2 and S^* when S^* has valence $|V|$ produces an increment in the absolute cathexis of S_2. The derived cathexis is an increasing decelerated function of the number of contiguities and the asymptote is an increasing decelerated function of $|V|$ during the contiguities, and has the same sign as the V of S^*. The presentation of S_2 without S^*, or with S^* having had its absolute valence decreased, will produce a decrement in the induced cathexis of S_2.

7. *Induced Elicitor-Cathexis*: The acquisition of valence by an expectandum S_2 belonging to an existing expectancy $(S_1R_1S_2)$ induces a cathexis in the elicitor S_1, the strength of the induced cathexis being a decelerated increasing function of the strength of the expectancy and the absolute valence of S_2.

8. *Confirmed Elicitor-Cathexis*: The confirmation of an expectancy $(S_1R_1S_2)$, i.e., the occurrence of the sequence $(S_1 \rightarrow R_1 \rightarrow S_2)$ when $(S_1R_1S_2)$ is of nonzero strength, when S_2 has a certain valence, produces an increment in the cathexis of the elicitor S_1.

This increment in the elicitor-cathexis by *confirmation* is greater than the increment which would be *induced* by producing a valence in S_2 when the expectancy is at the same strength as that reached by the present confirmation.

9. *Valence*: The valence of a stimulus S^* is a multiplicative function of the *need* D and the *cathexis* C^*. (Applies only to cases of positive cathexis.)

10. *Need Strength*: The need (D) for a cathected situation is an increasing function of the time-interval since satiation for it.

Note: Upon present evidence, even basic questions of monotony and acceleration are unsettled for the alimentary drives of the rat, let alone other drives and other species. There is no very cogent evidence that all or even most "needs" rise as a function of time since satiation, although this seems frequently assumed. Even the notion of satiation itself, in connection with "simple" alimentary drives, presents great difficulties. This proposition can, therefore, hardly be taken as having any generality even tentatively.

11. *Cathexis*: The cathexis of a stimulus situation S^* is an increasing decelerated function of the number of contiguities between it and the occurrences of the consummatory response. The asymptote is an increasing function of the need strength present during these contiguities. (There may be some innately determined cathexes, however.)

12. *Activation*: The reaction potential ${}_sE_R$ of a response R_1 in the presence of S_1 is a multiplicative function of the strength of the expectancy $(S_1R_1S_2)$ and the valence (retaining sign!) of the expectandum. There are momentary oscillations of reaction potential about this value ${}_sE_R$, the frequency distribution being at least unimodal in form. The oscillations of two different ${}_sE_R$'s are treated as independent, and the response which is momentarily "ahead" is assumed to be emitted.

As a sample of "derivation" from these, let us consider first a study by Kendler (6) which yielded somewhat puzzling results from the Hullian point of view. Rats were run in a single T maze when both hungry and thirsty. On one side the goal box always contained food, on the other water, and in both boxes the consum-

matory response occurred repeatedly during the first phase. On the test run, some animals were made thirsty, the others hungry. "Appropriate" choices were made to a pronounced extent. Kendler points out that since both left and right choices had been consistently reinforced during the training runs, and since both hunger and thirst were present as cue variables for choices in both directions, it is not clear on Hullian principles why the differential choice is shown on the one-drive test runs. He discusses two possibilities, one in terms of the fractional goal response (suggested by Spence) and the other involving a rather radical modification of the usual Hullian reinforcement principles, such that the only drive cues which get connected to R are those which are reduced by the reinforcing operation that strengthens the ${}_{S}H_{R}$ in question (Guthrie?). The finding would be treated within the present frame as follows:

S_C: Choice-point stimulation
R_R: Right turn (leads to food)
R_L: Left turn (leads to water)
S_{RF}: Stimulation in right goal box, including that of eating
S_{LW}: Stimulation in left goal box, including that of drinking

Then during the first phase we have the sequences:

$$S_C \rightarrow R_R \rightarrow S_{RF}{}^{*}$$
$$S_C \rightarrow R_L \rightarrow S_{LW}{}^{*}.$$

Repetitions of these in balanced amounts lead to increased strengths of two expectancies

$$(S_C R_L S_{LW}) \doteq (S_C R_R S_{RF})$$

according to (1). Unless the two drives are unequal or the number of exposures unbalanced, these two expectancies will rise at the same rate; toward the end of the first phase they should, on the average, be equal in cumulated strength. Assuming hunger and thirst drives to be equal, at this stage, by (10), (11), and (12), we have

$$\begin{aligned} {}_{S}E_{R(L)} &= (V_W)(S_C R_L S_{LW}{}^{*}) \\ &\doteq (V_F)(S_C R_R S_{FR}{}^{*}) = {}_{S}E_{R(R)} \end{aligned}$$

so no "preference" is manifested. On the test run, we satiate for food and keep the animals thirsty. That is,

$$(V_W) \gg (V_F)$$

so that

$$\begin{aligned} {}_{S}E_{R(L)} &= (V_W)(S_C R_L S_{LW}{}^{*}) \\ &\gg (V_F)(S_C R_R S_{RF}{}^{*}) = {}_{S}E_{R(R)}. \end{aligned}$$

By (12) the probability of a left turn is then much higher than for a right. It is instructive to ask where we find the *locus* of the difference that generates the derivation for us more readily than for Kendler. It evidently lies in the fact that *our activation postulate makes reference to the expectandum*, and this reference mediates a "control" over the strength of R which cannot be readily talked about in a notation designating the basic cognitive element by reference to S_1, the elicitor, and R only. Thus, the acquisition phase in Hullian terms is strengthening ${}_{S(C)}H_{R(R)}$ and ${}_{S(C)}H_{R(L)}$ equally, since turns both ways are reinforced. When it comes to predicting the test run, manipulating drive can only affect a multiplier of these ${}_{S}H_{R}$ elements, and the "forward-pointing" reference of the expectandum term is lacking or must be smuggled in by invoking r_g. In short, the Hullian frame makes the incentive important only "historically," i.e., in determining how much ${}_{S}H_{R}$ is cumulated. The expectancy frame retains the reference to the expectandum in the basic cognitive element ($S_1 R S_2$), and thus a reference to this third thing can be packed into

the activation postulate in a way that allows subsequent drive manipulations a ready control over the response strength.

Consider next the experiment reported by Tolman and Gleitman (**15**). After a first phase in which they ran either to left or right to readily discriminable goal boxes, in each of which they were allowed to eat, the rats were then *placed* (not following a run) in one of the goal boxes where they were allowed to eat. Each rat was also placed in the other goal box, where a shock was administered. On the test run the rats chose appropriately to a very striking extent. Suppose shock is administered in the left-hand goal box during Phase II, and food given in the right. Let the entire stimulus complex involving eating food be designated $S_F{}^*$ and that involving shock by $S_{Sh}{}^-$. Then the reported effects could arise as follows:

I. Status at end of Phase I:

a. $(S_C R_L S_L) \doteq (S_C R_R S_R)$ By (1)

b. $(S_C R_L S_F{}^*) \doteq (S_C R_R S_F{}^*)$ By (1)

c. $V_{S(L)} \doteq V_{S(R)}$ By (6)

II. Status at end of Phase II:

d. $(S_C R_L S_L) \doteq (S_C R_R S_R)$

e. $(S_C R_L S_F{}^*) < (S_C R_R S_F{}^*)$ By (4)

f. $(S_C R_L S_{Sh}{}^-) \gg (S_C R_R S_{Sh}{}^-)$ By (4) and (5), inference and incomplete generalization.

g. $V_{S(L)} \ll V_{S(R)}$ By (6), both through extinction of the linkage $S_L S_F{}^*$ and establishing that of $S_L S_{Sh}{}^-$.

III. On the test run, considering the resulting potentials,

h. ${}_sE_{R(L)} = (V_{S(L)})(S_C R_L S_L) \ll (V_{S(R)})(S_C R_R S_R) = {}_sE_{R(R)}$ By (12) with (*g*).

i. ${}_sE_{R(L)} = (V_{S(F)}{}^*)(S_C R_L S_F{}^*) < (V_{S(F)}{}^*)(S_C R_R S_F{}^*) = {}_sE_{R(R)}$ By (12) with (*e*).

j. ${}_sE_{R(L)} = (V_{S(Sh)}{}^-)(S_C R_L S_S{}^-) < (V_{S(Sh)}{}^-)(S_C R_R S_{Sh}{}^-) = {}_sE_{R(R)}$ By (12) with (*f*).

Now whatever may be the laws of summation for reaction potentials sharing elicitor and R term, so long as it is some increasing function of the components, it is clear that

$$ {}_sE_{R(L)} < {}_sE_{R(R)} $$

which is the desired result.

Consider as a final example the Spence-Lippitt type of latent-learning design reported as positive by those authors in 1940 (**13**) and by us in 1947 (**8**). Rats are run in a satiated state to goal boxes which, however, contain food or water. On the test run a state of either hunger or thirst is induced, and the appropriate choice tends to occur. Suppose food is on the right, water on the left. Then the sequences which occur during the satiated phase are

$$ S_C \rightarrow R_R \rightarrow S_R S_F{}^* $$
$$ S_C \rightarrow R_L \rightarrow S_L S_W{}^* $$

which will mnemonize two expect-

ancies ($S_C R_R S_R S_F^*$) and ($S_C R_L S_L S_W^*$) provided that "removal" is reinforcing (using [6] and possibly [7]). An additional contribution to the growth parameter might be made if we interpret (9) to mean a function of the form $V = a + b\,(D)(C^*)$ rather than of the form $V = b(D)(C^*)$, so that some valence persists for a cathected stimulus even when the need or drive variable is effectively zero. This is the approximate analogue in an expectancy theory to the claim in S-R-reinforcement theory that a stimulus retains its secondary reinforcing properties when the primary drive is satiated. That question, formulable in either system, is presently awaiting clear-cut solution experimentally (cf. **3, 4, 7, 10, 11**).

So after N trials of about equal runs to both sides,

$$(S_C R_L S_L S_W^*) \doteq (S_C R_R S_R S_F^*) \quad \text{by (1).}$$

And, since

$$(V_{S(W)}^*) \doteq (V_{S(F)}^*)$$

(assuming approximately equal preexperimental experience with food and water and current near satiation for each) we have by the activation postulate

$$\begin{aligned} {}_sE_{R(L)} &= (V_{S(L)S(W)}^*)(S_C R_L S_L S_W^*) \\ &\doteq (V_{S(R)S(F)}^*)(S_C R_R S_R S_F^*) \\ &= {}_sE_{R(R)}. \end{aligned}$$

We now manipulate drive, making the animal very hungry but not thirsty. Then

$$V_{S(W)}^* \ll V_{S(F)}^* \qquad \text{By (9)}$$

since

$$\begin{aligned} V_{S(W)}^* &= (D_W)(C_W)^* \\ &\ll (D_F)(C_F)^* = V_{S(F)}^* \end{aligned}$$

assuming from past history and (11) that

$$C_W^* \doteq C_F^*.$$

Then, by the activation postulate,

$$\begin{aligned} {}_sE_{R(L)} &= (V_{S(L)S(W)}^*)(S_C R_L S_L S_W) \\ &< (V_{S(R)S(F)}^*)(S_C R_R S_R S_F) \\ &= {}_sE_{R(R)}. \end{aligned}$$

There are certain additional consequences of the derivation that are of interest. To mention some of them briefly:

1. For a given valence the difference in ${}_sE_R$ is a function of the magnitude of the expectancies, since

$$\begin{aligned} {}_sE_{R(L)} - {}_sE_{R(R)} \\ = (V_L - V_R)(S_C RS) \end{aligned}$$

where

$$S_C R_R S_L = S_C R_R S_R = S_C RS.$$

a. But the magnitude of the expectancy increases with occurrences of the sequence, so that the percentage of appropriate responses on the test run should be less for rats who have had fewer "latent" runs during the satiated phase.

b. The growth rate of each expectancy depends on the valence of its expectandum (Mnemonization Law). For a constant number of cognition runs, the expectancy levels reached should be raised by any procedure that raises the valence of the goal boxes, even though *equally*. Thus, if another rat or home cage or preferred illumination is found in *both* boxes during the latent phase, this should raise the percentage correct on the test run.

However, plotting percentage "correct" on the test run against *number* of runs for groups differing in the valence of goal boxes should yield a family of curves of different slope but all approaching the same asymptote.

c. If the two goal boxes are varied from run to run so that the "modal" goal box on a side occurs randomly with a frequency P (and the presentation of food and water occur consist-

ently in this box) the percentage of correct choices on the test run is a function of P. The members of the family of curves in this case should approach different asymptotes.

2. For a given magnitude of expectancy the ${}_sE_R$ difference is a function of the valence difference. If both valences are small, ${}_sE_{R_R} - {}_sE_{R_L} = (V_R - V_L)(S_1RS_2)$ will be smaller than if one valence is large and the other small.

By the Law of Cathexis (11), both the cathexis of food and water will de minimal for rats that have never been on deprivation schedules for these commodities. As a result of this, by the Law of Mnemonization (1), expectancies for food on one side and water on the other will grow very slowly for such "naive" rats, and a short series of latent runs will, therefore, leave them with weak expectancies. Finally, by the Law of Valence (9), the valences of these expectanda will be minimal for such rats; and this is true both during the acquisition and test phases. Therefore, the ${}_sE_R$ difference between R_L and R_R on the test trial is doubly reduced by minimizing both the expectancy and valence terms in each (cf. Christie, **1**, **2**).

3. On the other hand, the *expectancy* strength approaches the same asymptote although more slowly, even for low valences (mnemonization law). If repeated occurrences of the sequence $S_1 \rightarrow R \rightarrow S_2$ have brought (S_1RS_2) close to this asymptote, we can then (outside the maze!) induce a strong cathexis in the alimentary expectanda by use of a feeding cycle. Such rats should (Law of Valence, Law of Activation) show the same per cent correct on a test trial as rats who had been hunger experienced prior to the experiment. But the asymptote of the expectancy (S_1RS_2) must have been reached.

Thus, for small numbers of "cognizing" runs, an interpolated hunger cycle (after the training series but before the test runs) ought not to bring these rats to the level of the previously hunger experienced. But for larger numbers of cognition runs, it should. The percentage correct on test runs plotted as a function of the number of training runs should approach the same asymptote for the interpolated group. But at the early phases of these curves a large difference is expected.

4. Since part of the valence of both S_R and S_L is assumed to be nonalimentary, based on the reward of removal, removal to a less valenced locus, e.g., mazelike box instead of home cage, or to a slightly negative-valenced locus such as a too-bright white box, should yield a slow growth of both expectancies. So for a small number of satiated runs, each expectancy has a lower value at the test run. Hence we expect less ${}_sE_R$ difference and more errors.

The obvious Hullian complaint would be, "Here you really admit the importance of the reward *during* the latent (satiated) phase." In a sense this is true. Yet, how does the reward *act* in the present formulation? It is not conceived of as generating a "habit," in favor of the later-to-be-chosen side. Suppose we were to give a *strong* but *equal* reward on both sides. The present derivation assumes that if we consider only rats having no bias to the left during the latent phase, they will still show the advantage in question on the test run. After all, the quantitative results of other studies indicate a very marked effect on response strength when an alimentary drive is raised from near zero to a 24- or 48-hour value. One would presumably be safe in predicting that the parameters of our equations are such that a slight

right-going bias (say, 10–20 per cent) manifested during satiated runs will be easily overcome by the valence shift on the test trial. Hullian postulates would in such a case merely raise the multiplication factor $f(D)$ and the algebraic sign of $({}_sE_{R(L)}) - ({}_sE_{R(R)})$ would still be negative. Barring the use of r_g, the predicted Hullian result is for such "biased" rats to manifest the bias with higher probability on the test run.

A concluding clarification of our own position seems necessary. The preceding has been presented as one way of formulating the expectancy position. We consider ourselves chiefly identified with some form of S-R-reinforcement theory, and our sympathies remain with it. The foregoing development is, therefore, *not* presented as empirically confirmed or even as a strong contender. Actually, the question of its factual adequacy has played almost no part in our thinking, which was directed at explicating one current theory rather than at proposing an empirically supported system with maximal attention to the facts.

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THE MATHEMATICAL FORMULATION OF A UNIFIED FIELD THEORY

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To what extent is it possible to incorporate such areas of psychology as classical conditioning, instrumental conditioning, perception, the Weber-Fechner laws, Helson's work on adaptation level, and certain aspects of the nervous system within a common mathematical frame of reference? Points relating to this problem will be offered through an analysis of the results of experiments and the discussions of Weber, Fechner, and Helson.

The author has stressed the fact in previous writings (**1**, **2**, **3**, **4**) that, under certain conditions, any stimulus might serve as a motivation and the reduction of that stimulus as a reinforcement to the organism. One of the experiments designed to test this point of view was performed by Caldwell and Mosman (**3**). In this experiment low temperature was utilized in a maze as motivation and high temperature in the goal box as reinforcement. Learning curves in terms of time and errors were established for albino mice. Another one of these experiments was performed by Caldwell and Sandler (**4**). This experiment consisted of using gaseous formaldehyde in a maze as motivation and its relative absence in the goal box as reinforcement. Learning curves were established for the albino mice utilized in this experiment.[2]

An advantage of a temperature-type experiment over the use of hunger and thirst as motivations is the possibility of measuring the stimulus in both its motivating and rewarding phases to a higher degree of accuracy. These experiments might be viewed from a psychophysical frame of reference where the concept of just noticeable difference is in a more dynamic setting than in terms of differences between stimuli. It might be thought of as differences between differences. The problem then may be stated as: *What is the increase in the stimulus in the maze minus the increase in the stimulus in the goal box necessary to produce a just noticeable difference in time and errors.*[3] It might be possible to find experimentally many of the differ-

[1] The author wishes to express his appreciation to John G. Tiedemann for editorial assistance in the preparation of this paper.

[2] A series of experiments utilizing different kinds of stimuli as motivation is being conducted by the author at the present time at The George Washington University comparative laboratory. The first of these consists of placing mice in a maze which is rotated about a central axis. When the animal reaches the center of the maze, the experimenter stops the rotation. The animal then remains in the center for ten minutes. The purpose here is to investigate the role of vestibular stimulation and its relative absence as motivation and reinforcement. The second of these experiments is similar to the one performed by Caldwell and Mosman except that the maze has a high temperature and the goal box has a low temperature. The third experiment consists of a maze which has a very bright light overhead and a very dark goal box. In all of these experiments control animals run the maze without the additional stimulus of temperature, light, or vestibular excitation. The ability to measure more accurately both qualitative and quantitative variations in the maze and in the goal box allows the experimenter the possibility of treating data from these experiments within a psychophysical framework.

[3] It is assumed that if the various combinations of temperature differences were tested, that different experimental and control animals would be used with each difference.

ences in temperature and construct a scale with the center being zero.

This same general approach might be applied to the design where formaldehyde was used as the stimulus, or to Jerome and Flynn's (**13**) experiment using light avoidance. It is this kind of experimental design which led the author to think of psychophysical phenomena in terms of differences between differences rather than differences between stimuli, or, to put it in other words, to think of stimulus as being a field with a difference in that field.

Helson (**8, 9, 10**) has been doing some very important theoretical and experimental work in the field of perception on the general problem of adaptation level and frames of reference. Helson (**8**, p. 2) defines adaptation level as follows: "For every excitation-response configuration there is assumed a stimulus which represents the pooled effect of all the stimuli and to which the organism may be said to be attuned or adapted." Helson deals with the problem of adaptation from many different angles. He emphasizes the effects of background upon judging stimuli. He also suggests many applications of this to social phenomena. Helson says:

> Thus if a weight is said to be heavy, or a light dim, or a painting beautiful it is because the object in question appears above or below the indifference point of functioning. Such points can be said to represent the adaptation-level of the organism with respect to given stimuli. We find, for example, that the neutral stimulus in a series of weights ranging from 200 to 400 grams is about 250 grams, while in a series ranging from 400 to 600 grams the neutral stimulus is about 475 grams. If a background or comparison stimulus of 900 grams is introduced, the neutral stimulus becomes 350 grams in the first case and 550 grams in the second case. The adaptation-level, in general, tends to be determined largely by the values of the series and background stimuli. The effects of past experience are usually not sufficient to displace the adaptation-level from within the stimulus continuum (**10**, p. 380).

Helson says:

> The concept of adaptation-level (AL) must not be restricted to the effects of prolonged adaptation to more or less constant stimulation with greatly reduced capacity for response as a final end-state. There is an AL for every moment of stimulation. It is a function of all the stimuli acting upon the organism at any given moment as well as in the past (**8**, p. 3).

Helson, in discussing the mathematical treatment of adaptation level, says:

> Choice of a logarithmic function is in keeping with the well-established Weber-Fechner law governing stimulus-response relationships over a fairly wide range as well as the law of diminishing returns operative in the wider field of social and economic behavior. The formula found adequate in vision proved to be:
>
> $$AL = K(A_0^3 \bar{A})^{\frac{1}{4}} \qquad (1)$$
>
> where K is a fractional constant, A_0 is the brightness of the background, and $\bar{A}$ is the logarithmic mean of the brightnesses of the samples on the background. From this formula it is seen that the AL is a weighted geometric mean in which background is loaded three times as heavily as the log mean of all samples in the field (**8**, p. 3).

Helson also discusses the relation between adaptation level and Weber's law wherein he says:

> If the judgment of a stimulus, X_i, depends upon its distance from adaptation-level, A, it seems reasonable to assume that the judgment is related to the number of just noticeable differences between A and X_i since "psychological" distance must be stated in psychophysical terms. We can derive psychological distance

from the physical magnitudes involved by dividing the stimulus distance, $X_i - A$, by the value of the j.n.d. in physical units also. According to Weber's law, the increment ΔX which must be added to obtain a just noticeable difference from a standard, X, is a constant fraction of X or:

$$\Delta X = kX, \qquad (1)$$

where k is the Weber constant. According to our basic assumption that judgments are made with respect to A, we should use this value as the base on which the Weber constant k operates. However, since a particular stimulus is being judged at any given time, we need to take its value into account also and hence we take the average of the two as the basis for a j.n.d. from adaptation-level giving equation (2):

$$\Delta A = k\,(X_i + A)/2. \qquad (2)$$

Equation (2) actually weights the stimulus immediately in perception and adaptation-level equally in determining the size of the j.n.d. and this, the simplest assumption regarding their relative importance, proves to be adequate for a wide variety of data (9, p. 302).

Weber dealt with stimuli in a somewhat static sense. His basic formula was:

$$\frac{\Delta st}{st} = K.$$

This formula stated verbally is: The just noticeable increase in a stimulus is a constant fraction of that stimulus. Fechner enlarged upon this conception and his psychophysical formula was:

$$S = C \log R.$$

This formula stated verbally means, sensations are proportional to the logarithms of their exciting stimuli.

An important theoretical question at this point is the possibility of constructing a mathematical formula which will integrate the work of Weber, Fechner, Helson, and the maze-learning type of experiments discussed earlier. Another problem would be the utilization of such a formula in bringing together the fields of perception, learning, and classical and instrumental conditioning under one quantitative frame of reference. It then seems logical to try to integrate present-day experimental findings in various fields with the historical work of Weber and Fechner.

In reviewing the temperature experiment previously discussed, the assumption is made that psychophysical phenomena operate within an adaptive framework. Possibly these temperature experiments reveal the adaptive nature of the process more clearly than do some other types of experiment. This same type of process could be involved in the weight-lifting experiments dealing with perceptual phenomena. For every weight that is lifted we might postulate from Helson's (10) experimental data on adaptation level, there will be a weight which weighs less and yet will elicit a judgment of equality from the subject. The physical difference in the above weights could correspond to the difference in temperature between the maze and the goal box. This frame of reference might also be applied to the classical conditioning type of experiments proposed by Pavlov and Liddell. In the classical conditioning technique, a neutral stimulus could be established for the conditioned stimulus which would be comparable to the neutral point in the temperature and weight experiments.

DISCUSSION OF A FIELD CONCEPT OF STIMULUS

The concept of stimulus has had many different definitions and applications in psychology. It is the purpose of this paper to emphasize the dynamic field aspects of the concept of stimulus. It is suggested here that,

operationally, stimulus should be thought of in two phases: one, the measure of the application of the stimulus, and, two, the measure of the stimulus after a stated period of adaptation by the organism to it. It is suggested that one j.n.d. below a stimulus should represent the stimulus in terms of the amount of adaptation exhibited by the organism. The theoretical viewpoint taken of the stimulus is that it is a field of energy represented by a difference acting upon an organism. One part of the field is the initial measure of the stimulus and the other part is at least one j.n.d. below the initial stimulus.

A TENTATIVE MATHEMATICAL FORMULATION

The following is a presentation and discussion of a tentative mathematical formulation of the theory of adaptive differentiation to be used as a tentative guide to experimental research.

Weber's formula, once again, is:

$$\frac{\Delta st}{st} = K.$$

A tentative revision of Weber's formula would be:

$$\frac{\Delta Si - \Delta Sj}{Si - Sj} = K,$$

where (*a*) *Si* represents the initial stimulus, (*b*) *Sj* represents one j.n.d. below the initial stimulus. The verbalization of this formula is as follows: *The increment of difference in a stimulus field represented by the difference between the increment of the initial stimulus minus the increment of the stimulus which is one j.n.d. below the initial stimulus to produce a just noticeable differentiation in the organismic field of energy is a constant fraction of the difference in the stimulus field represented by the difference between the initial stimulus and the stimulus which is one j.n.d. below it.*

Once again, Fechner's formula is:

$$S = C \log R.$$

A tentative revision of the Fechner formula would be:

$$d = C \log (Si - Sj),$$

where (*a*) *d* represents organismic differentiations, (*b*) *Si* represents the initial stimulus, (*c*) *Sj* represents one j.n.d. below the initial stimulus, (*d*) *C* represents a constant. The verbalization of this formula is as follows: *Organismic differentiations equal a constant times the logarithm of the difference in the stimulus field of energy represented by the difference between the initial stimulus and a stimulus which is one j.n.d. below the initial stimulus.*

SOME TENTATIVE POSTULATES

The following are some postulates utilizing the above mathematical formulation.

I (A) *The increment of the difference between the stimulus in a maze and a stimulus in the goal box which is one j.n.d. below the stimulus in the maze to produce a just noticeable difference in time and errors is a constant fraction of the difference between the stimulus in the maze and the stimulus in the goal box which is one j.n.d. below the stimulus in the maze.*

(B) *Differentiations in maze situations in terms of time and errors are equal to a constant times the logarithm of the difference.*

The formula for (A) is:

$$\frac{\Delta MSi - \Delta GSj}{MSi - GSj} = K,$$

where (*a*) *MSi* represents the stimulus in the maze, (*b*) *GSj* represents the stimulus in the goal box which is one j.n.d. below the stimulus in the maze.

The formula for (B) is:

$$MD = C \log (MSi - GSj),$$

where (*a*) *MD* represents maze differentiations in terms of time and errors, (*b*) *MSi* represents the stimulus in the maze, (*c*) *GSj* represents the stimulus in the goal box which is one j.n.d. below the stimulus in the maze.

An operational definition of GSj. *GSj* is obtained by utilizing time and errors rather than sensation or judgments in psychophysical experiments. The problem is, *what is the least difference between the maze stimulus and the goal stimulus to produce a just noticeable difference in time and errors from that which would appear by chance or in a situation where there was no difference between the stimulus in the maze and the stimulus in the goal box.* In other words, there should be a statistically significant difference in time and errors between the experimental situation where there is a difference between maze and the goal box and the experimental situation where there is no difference between the maze and the goal stimulus.

II (A) *The increment of the difference between an initial conditioned stimulus and a conditioned stimulus which is one j.n.d. below it to produce a just noticeable differentiation (utilizing a conditioned response in animals as a substitute for judgment) of stimuli is a constant fraction of the difference between the initial conditioned stimulus and the conditioned stimulus which is one j.n.d. below the initial conditioned stimulus.*[4]

(B) *Differentiations of stimuli utilizing conditioned responses as judgments equal a constant times the logarithm of the difference between the initial conditioned stimulus and the conditioned stimulus which is one j.n.d. below it.*

The formula for (A) is:

$$\frac{\Delta CSi - \Delta CSj}{CSi - CSj} = K,$$

where (*a*) *CSi* represents the initial conditioned stimulus, (*b*) *CSj* represents the conditioned stimulus which is one j.n.d. below the initial stimulus.

The formula for (B) is:

$$CRD = C \log (CSi - CSj),$$

where (*a*) *CRD* represents differentiation of stimulus where conditioned responses are substituted for an act of judgment, (*b*) *CSi* represents the initial conditioned stimulus, (*c*) *CSj* represents the conditioned stimulus which is one j.n.d. below the initial conditioned stimulus.

III (A) *The increment of the difference between a stimulus and a stimulus which is one j.n.d. below the initial stimulus for the organism to elicit a just noticeable differentiation utilizing an instrumental conditioned response as the equivalent of judgment is a constant fraction of the difference between the initial stimulus and a stimulus which is one j.n.d. below it.*

(B) *Differentiations in terms of instrumental conditioned responses equal a constant times the logarithm of the difference between the initial conditioned stimulus and the conditioned stimulus which is one j.n.d. below the initial conditioned stimulus.*

The formula for (A) is:

$$\frac{\Delta Si - \Delta Sj}{Si - Sj} = K,$$

where (*a*) *Si* represents the initial instrumental conditioned stimulus, (*b*) *Sj* represents the instrumental conditioned stimulus which is one j.n.d. below the initial stimulus.

The formula for (B) is:

$$ICRD = C \log (Si - Sj),$$

[4] This has reference to the experiments where animals are trained to discriminate between different tones, etc., using the conditioned response as the index of the difference.

where (*a*) *ICRD* represents instrumental response differentiations, (*b*) *Si* represents the initial instrumental conditioned stimulus, (*c*) *Sj* represents the stimulus which is one j.n.d. below the initial stimulus, (*d*) *C* represents a constant.

IV (A) *The increase in a difference between a stimulus and a stimulus which is one j.n.d. below the initial stimulus to produce a just noticeable differentiation of lifted weights is a constant fraction of the difference between the stimulus and the stimulus which is one j.n.d. below the initial stimulus.*

(B) *Differentiations of lifted weights equal a constant times the logarithm of the difference between the initial stimulus and the stimulus which is one j.n.d. below the initial stimulus.*

The formula for (A) is:

$$\frac{\Delta Si - \Delta Sj}{Si - Sj} = K,$$

where (*a*) *Si* represents the initial stimulus, (*b*) *Sj* represents the stimulus which is one j.n.d. below the initial stimulus.

The formula for (B) is:

$$DW = C \log (Si - Sj),$$

where (*a*) *DW* represents the differentiations of lifted weights, (*b*) *Si* represents the initial stimulus, (*c*) *Sj* represents one j.n.d. below the initial stimulus, (*d*) *C* represents a constant.

V (A) *The increment of the difference between the intensity of a stimulus of constant duration and the intensity of the stimulus of constant duration one j.n.d. below the initial intensity of the stimulus to produce a just noticeable difference in the frequency of nerve impulses within a single nerve fiber is a constant fraction of the difference between the initial intensity of the stimulus of constant duration and the intensity of the stimulus one j.n.d. below the initial intensity of the stimulus.*

(B) *The frequency of nerve impulses is equal to a constant times the logarithm of the difference between the initial intensity of a stimulus of constant time interval and the intensity of the stimulus which is one j.n.d. below the initial stimulus intensity.*

The formula for (A) is:

$$\frac{\Delta I_1 - \Delta I_2}{I_1 - I_2} = K,$$

where (*a*) I_1 represents the initial intensity of a stimulus of constant time interval, (*b*) I_2 represents the intensity of the stimulus which is one j.n.d. below the initial stimulus.

The formula for (B) is:

$$FNI = C \log (I_1 - I_2),$$

where (*a*) *FNI* represents the frequency of nerve impulses, (*b*) I_1 represents the initial intensity of the stimulus of constant time interval, (*c*) I_2 represents the intensity of the stimulus which is one j.n.d. below the initial stimulus, (*d*) *C* represents a constant.

VI (A) *The increment of difference between the time interval of a stimulus of constant intensity and the time interval one j.n.d. below the initial time interval of the stimulus to produce a just noticeable difference in the frequency of impulses within a single nerve fiber is a constant fraction of the difference between the initial time interval of the stimulus of constant intensity and the time interval one j.n.d. below the initial time interval of the stimulus.*

(B) *The frequency of nerve impulses equals a constant times the logarithm of the difference between the initial time interval of a stimulus of constant intensity and the stimulus which is one j.n.d. below the initial time interval of the stimulus of constant intensity.*

The formula for (A) is:

$$\frac{\Delta T_1 - \Delta T_2}{T_1 - T_2} = K,$$

where (*a*) T_1 represents the initial time of a stimulus of constant intensity, (*b*) T_2 represents the time interval which is one j.n.d. below the time interval of the stimulus of constant intensity.

The formula for (B) is:

$$FNI = C \log (T_1 - T_2),$$

where (*a*) *FNI* represents the frequency of nerve impulses, (*b*) T_1 represents the initial time of a stimulus of constant intensity, (*c*) T_2 represents the time interval which is one j.n.d. below the time interval of the stimulus of constant intensity, (*d*) *C* represents a constant.

Some Applications of This Formulation to Existing Psychophysical Data

Most of the preceding postulates can be fitted to general psychophysical data. A simple example illustrating the application of previous data to this reinterpretation of the Weber-Fechner fraction can be taken from Garrett (**5**, pp. 334–335). In this experiment there are two stimuli, a standard and a variable comparison stimulus. These two stimuli might be lines, lights, tones, etc. The two stimuli have a value of 100 each. If the comparison stimulus is increased until a value of 110 is reached, it is judged to be just noticeably different from the standard. The Weber fraction is 1/10.

The following are some calculations of j.n.d.'s based upon this particular fraction:

1. 100
2. 110
3. 121
4. 133
5. 146.41
6. 161.05
7. 177.16
8. 194.87
9. 214.36
10. 235.79
11. 259.37

Now, if the formula

$$\frac{\Delta Si - \Delta Sj}{Si - Sj} = K$$

is applied between steps two and three or the figures, 110 and 121, the judgment in terms of this present theory would be from 1 to 2 or from 100 to 110 respectively. The substitution in this formula would be:

$$\frac{11 - 10}{110 - 100} = \frac{1}{10}.$$

From the standpoint of the organism, one may think of stimuli which are obtained by the Weber-Fechner fraction as ascending or descending in *pairs* and not as separate stimuli. The calculation now allows this reinterpretation to fit any data where the Weber-Fechner fraction has been found to hold.

In many applications of this mathematical formulation the supporting evidence will come from already existing psychophysical data and the problem is merely reinterpreting the data in terms of a more dynamic point of view.

For postulate II on classical conditioning, the experimental work of Hovland (**12**), where psychophysical methods were applied to the conditioned galvanic skin response, may be utilized. For postulate III on instrumental conditioning, reference may be made to Lashley (**14**). For postulate IV on perception, one example has been discussed and others include standard weight-lifting experiments, etc. For postulate V on the frequency of nerve impulses in relation to intensity of stimulation, reference may be made to the data from

Hodgkin (11) where the relationship between the intensity of a stimulus and the frequency of impulses was investigated in a single motor fiber from a crab. For postulate VI on the relationship between duration of the stimulus and its relation to the frequency of the impulse, reference should be made to the work of Hartline (6) and Hartline and Graham (7) where they studied the impulse discharge of single photoreceptors of the horseshoe crab. Postulate I on maze learning may be tested by further testings of temperature differences in the experiment performed by Caldwell and Mosman (3).

SUMMARY AND IMPLICATIONS

The previous postulates represent an attempted homogenization of somewhat diverse fields in psychology. It should be kept in mind that this formulation is a somewhat ideal one and would be subject to the same limitations and criticisms that have been applied to the Weber-Fechner fraction. Also, in actual applications of it, particularly outside of ideal laboratory conditions, the work of Helson on background stimuli may be important.

The question may be raised as to the value of such a mathematical formulation as presented herein. The following are some suggestions pertaining to its value. It allows:

1. greater semantic unification of psychological theory;
2. a more precise quantification of field theory;
3. a psychophysical treatment of motivation, learning, and reinforcement;
4. mathematical proof of the similarities between classical and instrumental conditioning;
5. a more universal application of Helson's concept of adaptation level;
6. for more specific, quantitatively measurable hypotheses for research;
7. a dynamic interpretation of the Weber-Fechner law;
8. the increasingly important concept of homeostasis to be treated quantitatively.

The previous formulations were presented with the idea of suggesting areas of research. This theory termed *adaptive differentiation* and its mathematical formulation were presented, not as something new, but rather as a new organization with the idea that it might further stimulate experimenters and theorists to focus on the problem of qualitative and quantitative unification.

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ADDITIVE SCALES AND STATISTICS

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Psychological measurements do not possess the simple properties of the scales obtained for those basic dimensions of physics which have been designated as "fundamental magnitudes." The implications of this statement for quantitative psychology have been extensively studied and discussed with varying evaluations and recommendations. Frequently the recommendations have been such as to alter statistical practices, had they been followed.

Certain writers, notably Boring (2) and Stevens (6), have maintained that such statistical concepts as the sample mean and standard deviation presuppose, at the very least, a scale of equivalent units of some kind, thus casting doubt on the theoretical validity of extensive reliance on the *t* test, analysis of variance, and other statistical techniques widely used with psychological data. The resulting distrust of such widely used procedures has prompted Comrey (4) to seek their justification outside the strict limits of the traditional logic of measurement.

It is the purpose of the present paper to analyze this issue and to show that the use of the sample mean and standard deviation does no violence upon the data, whatever the properties of the measurement scale. Thus, the use of the usual statistical tests is limited only by the well-known statistical restrictions.

The argument to be given can be conducted from the axioms of probability and the axiomatic basis of measurement, but such detailed treatment would be merely pretentious, since the results which are necessary to establish the basic point are familiar to almost all psychologists.

The Nature of Measurement Scales

(The term "object" as used below should not be restricted to its usual meaning of "physical object"; rather it is to be interpreted with sufficient breadth to give the statements throughout this section meaning for psychological as well as physical measurement provided that the axioms can be satisfied.)

Objects which can be ordered on the basis of a pair of (physical, psychological, or other) relations are said to define a "dimension." For such objects there are two relations, objectual equality and objectual less-than-ness and the objects and relations satisfy the axioms of order reproduced by Comrey (4). Each object can be tagged with a number so that the numbers will satisfy a corresponding set of axioms. Thus there is a correspondence between the two systems:

(A) [Objects, objectual equality, objectual less-than-ness.]

(B) [Numbers, numerical equality, numerical less-than-ness.]

For some objects and relations, a further step is possible. An operation for combining the objects, "objectual addition," can be found such that the system:

(C) [Objects, objectual equality, objectual less-than-ness, objectual addition]

satisfies four additional axioms of combination (Comrey [4]). When this is the case the objects can be tagged with numbers so that the system:

(D) [Numbers, numerical equality, numerical less-than-ness, numerical addition]

satisfies four corresponding additional axioms of combination. Thus, in this case, there is a correspondence between (C) and (D).

When the systems (C) and (D) exist and correspond, we say that the objects define an "extensive dimension" and the numbers an "additive scale." In this case, of course, the systems (A) and (B) also exist and correspond.

When the systems (A) and (B) exist and correspond, but the systems (C) and (D) do not, we say that the objects define an "intensive dimension" and the numbers a "rank-order scale."

These matters are discussed in great detail by Campbell (3) and, more adequately for psychologists, by Bergmann and Spence (1). Pertinent information is presented in papers by Comrey (4) and Gulliksen (5). For our purposes, it is important to note only that the classification of a scale as additive depends upon the presence or absence of a certain correspondence, expressed in sets of axioms, between the numbers of the scale and the objects to which they refer—with, of course, appropriate ordering relations and combinative operations for each.

THE NATURE OF STATISTICS

Statistical methods serve two major functions for psychologists.

a. They are used to summarize the salient features of individual sets of data.

b. They are used to test for differences between different experimental groups.

We shall discuss the second function in some detail, restricting our discussion to the simple case in which two groups are compared. In the typical psychological experiment the operations performed by the experimenter yield two or more sets of numbers. (In fact, unless the data exist in numerical form, means and standard deviations cannot be computed and the data are irrelevant for the present discussion.) It is obvious that two experimental groups will be judged alike or different in a given respect according as the collections of numbers classifying them in this respect are judged to be alike or different. It should be emphasized that we are here comparing the two sets of numbers *as numbers* and nothing else about them matters until after the statistical test has been made. The application of statistical techniques reflects merely our recognition of the unreliability of the small sets of numbers we have obtained and our unwillingness to perform the experiment again and again to determine whether the direction of the difference between our groups is reliable. We conceptualize a larger set of numbers, the statistical population, from which the sets of numbers we have obtained are two small samples, and inquire into the likelihood of two samples as disparate as we have observed arising from the given population. In answering this question, we often use the sample means and standard deviations as indices of important aspects of our collections of numbers. No interpretation other than this indicial one is intended. Means and standard deviations are used because they can always be computed, since numbers can always be added, subtracted, multiplied, and divided, and because means and standard deviations, conceived of merely as the results of operations with numbers, behave in certain lawful statistical ways.

In summary, the statistical technique begins and ends with the numbers and with statements about them. The psychological interpretation given to the experiment does take cognizance of the origin of the numbers but this is irrelevant for the statistical test as such.

Obviously, the same argument applies directly to the first function of statistics as well. The statement "The mean of these scores is 121" conveys in general

the same kind of information as the statement "The median of these scores is 122."

The objection that a well-established unit is necessary before the mean and standard deviation can be computed since their value is altered by a change in the absolute value of the scores (Comrey [4]) loses cogency when one notices that the mean and median will be affected in precisely the same way by adding a given number to each number in the sample and that the standard deviation and the interquartile range will be changed in the same way by multiplying each number in the sample by a given number.

AN EXAMPLE

To establish the point in another way, we consider an example of a statistical test based on an additive measure. Suppose that we are presented with two sticks, A and B, of apparently equal lengths, fixed on opposite sides of a room, and asked which is longer. We measure them and obtain a larger number for A. The two numbers, however, are nearly the same and we decide to repeat the measurement "just to make sure." On this occasion, we obtain a larger value for B. We proceed until we have 100 measurements on each stick and wish to answer the question without taking further measurements.

There are two collections of 100 numbers each, one for stick A and one for stick B. We test the hypothesis that they differ only through the unreliability of the measurements. A moment's reflection will show that we are not at all concerned with the additive nature of the scale for length. In adding the 100 numbers to obtain a mean for the measurements on stick A, we treat them as numbers and as nothing else. We make no interpretations whatever about adding 100 sticks together—there are only two sticks. Moreover, our interpretation by means of the t test is unaffected by the choice of length units we have made, provided that the units are the same for the two sticks.

It is seen that the comparison of the sets of measurements on the two sticks differs in no essential way from the comparison of two sets of IQ's.

SUMMARY AND CONCLUSIONS

We have noted that: (a) The properties of a scale of measurement involve correspondences between sets of axioms about objects and numbers, with appropriate relations and operations. (b) Statistical methods begin and end with numbers.

From (a) and (b), we have deduced that the properties of a set of numbers as a measurement scale should have no effect upon the choice of statistical techniques for representing and interpreting the numbers.

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THE AMES OSCILLATORY EFFECT: A REPLY TO PASTORE

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Pastore's (3) critical discussion in this journal of a portion of Ames's (1) monograph on the rotating trapezoid has prompted this rejoinder based on close association with Ames during the time he was preparing his monograph and on a number of my experiments in which the rotating trapezoid was employed as basic apparatus (2). Discussion will be confined to Pastore's comments concerning the generality of the oscillatory phenomenon, and to his asserted disagreement with Ames's explanation.

With regard to the generality of the phenomenon, Pastore says, "Ames . . . implies that the perceived oscillatory motion is unique to the trapezoidal window. The writer [Pastore], however, found that the oscillatory effect could be obtained with a wide range of forms" (3, p. 319). Ames will no doubt be horrified that such an implication is to be found in his paper, but a careful reading of it would seem to mark him as at least partially guilty on that count. The implication is unintentional, however, as Ames experimented with a great variety of forms which, when rotated, yield some degree of apparent oscillation. A number of these forms are described in the appendix to his paper (1). Ames's final selection of a form was based on his desire to maximize the amount and stability of the apparent oscillation so the phenomenon could be studied more conveniently and so it would not break down easily when placed in conflict with other moving objects. In his concentration on these ends, Ames failed adequately to point out the generality of the phenomenon, and Pastore must be credited with having performed a most necessary service.

However, it should be noted that such generality covering such a very wide range of forms as that described by Pastore is probably an artifact of Pastore's method. Pastore says, "Although the writer used smaller figures than did Ames, it is most likely that similar results will be obtained if larger figures were employed" (3, p. 319–20). On the contrary, it is most unlikely that similar results will be obtained if observation distance remains the same. Pastore's assertion overlooks the vital roles played by changing horizontal and vertical visual angles in the perception of either oscillation or rotation. Let us suppose that a rectangular plane figure is rotated once completely, starting at a position normal to the line of sight (0°). The horizontal visual angle (or the horizontal dimension of the retinal pattern) is at its maximum at 0°, diminishes until it is at a minimum at 90°, when the figure is seen end-on, increases to its maximum at 180°, decreases to its minimum at 270°, and finally reaches its maximum again at 360°. Exactly the same changes take place if the plane is turned to 180° and then reversed to 0°; that is, if it oscillates. Additional "information" is needed, then, to distinguish rotation and oscillation. The possible sources of information are many, but in the case of objects with relatively homogeneous surfaces and balanced lighting (such as those used by Pastore), the most important source is the asymmetrical alterations in the vertical dimensions of the retinal image. These are not the same for rotation and oscillation, and in most cases serve very well in differentiating the two. However, to the degree that the figures are made small or ob-

servation distance is increased, these asymmetrical alterations in the vertical dimensions of the retinal image become less effective, and the motion of the figure becomes more ambiguous. It may be seen either as oscillating or as rotating.

Pastore gives us little data concerning figure size or observation distance, but if the 1½″ × 5″ figure and the 35-foot observation distance he gives in one instance may be taken as representative, it is almost certain that many of his results may be accounted for in terms of the above analysis. This is not the phenomenon described by Ames. The illusory oscillation he described is quite unequivocal with a large figure at a near distance; in fact, within limits, and for monocular observation, the larger the figure and the nearer the distance, the more unequivocal it becomes. Ames used in his laboratory work a figure 19½″ long, 23⅝″ high at one side, and 12½″ high at the other, with a viewing distance of about 10 feet (monocular). With this apparatus most observers report that some of the illusory effects are still present even when they move forward to a distance of 5 or 6 feet. Clearly, some other explanation is called for, and that is what Ames endeavored to supply in his monograph.

It is with this explanation that Pastore says he disagrees. But does he disagree? Ames says,

> As the trapezoidal window starts to rotate . . . [from a position normal to the line of sight with the short edge to the left] . . . , the total horizontal angle that the trapezoidal window subtends to the eye decreases. At the beginning of this decrease the trapezoidal window appears tipped back on the left. It has been learned from past experience with rectangular forms that a decrease of the total horizontal angle of our retinal images of a rectangularly perceived form which appears tipped away from us on the left could only take place if the side on the left went farther away. If it came nearer, the total horizontal angle of our retinal images would have to increase. So we interpret this decrease in the total horizontal dimension of our retinal stimulus pattern as a going-away of the left side of the window. That is, the window appears to reverse its direction of rotation and as the left side of the window keeps coming toward us, it appears to be going farther away. A similar apparent reversal is seen to take place when the trapezoidal window has revolved to a position where the short side . . . is to the right (1, pp. 15–16).

The heart of Pastore's "alternative interpretation" which ". . . seems to be in closer accord with the facts and also enables one to predict the occurrence of the oscillation effect" (3, p. 321) is as follows:

> Let us now return to the original assumed position of the trapezoidal figure—viz., at a tilt of plus 45 degrees. It so happens that the usual *O* will perceive the tilt to be not plus 45 degrees but some negative acute angle. That is to say, the long edge of the figure will be seen as being closer than the shorter edge. Despite the fact that the perceived tilt does not coincide with the actual tilt, the retinal projection of the rotating trapezoidal figure must remain an expanding one. In the phenomenal experience of *O* this could only be the case when the perceived direction of rotation of the trapezoidal figure is counter-clockwise (3, p. 321).

Ames chose for his illustration the portion of the figure's rotation when the retinal image is contracting; Pastore chose the portion when it is expanding. Otherwise the explanations are identical, unless Pastore means by "the phenomenal experience of *O*" something other than the sort of experience specified by Ames.

It may be that he does mean something different, because in one instance (3, p. 320) he quotes a statement by Ames concerning the nature of the experience involved, and then asks, "But

how could the individual, in his actions undertaken in an environment in which the retinal impression of rectilinear[1] forms is most frequently nonrectilinear, form an assumption about rectilinearity which is claimed to be so effective in shaping the sensory material? Why not form an assumption of 'trapezoidalness'?" (3, pp. 320–21). These questions indicate a misunderstanding of Ames's point. Ames does not suggest that the crucial experience is with a retinal pattern; he does not imply that the observer is looking at his own retina. On the contrary, the crucial experience is that of manipulating, dealing with, acting toward rectangular forms. It seems likely that we would, indeed, form assumptions of trapezoidalness if we lived in a world of trapezoidal forms.

Perhaps the key to this misunderstanding lies in what appears to be a preference by Pastore (3, pp. 321, 323) for a purely Gestalt explanation divorced from empiricism. If so, the issue is a clear one. Ames would agree ". . . that the preferred perceptual tendency is the outcome of the distribution of central processes (which are functionally related to retinal impressions) . . ." (3, p. 321), but would argue that the distribution and functional relationship must be accounted for mainly in terms of past experience. If this is the true issue, however, one still wonders what Pastore means by "the phenomenal experience of *O*."

Another matter regarding Pastore's "alternative interpretation" should be mentioned. He says, "One point of the interpretation proposed by the writer [Pastore] which remains unexplained concerns the factors responsible for a perceived surface whose tilt differs from the actual surface" (3, p. 323), thus giving the impression that the point is not central to Ames's explanation and perhaps was not even considered by Ames. On the contrary, Ames explicitly raised this point (1, pp. 7, 13, 16) and dealt with it at length. In fact, he even went to the trouble of preparing a 49-figure chart (1, p. 6) whose major function is to illustrate his discussion of that particular problem.

Incidentally, Pastore errs in his description of the role played by the discrepancy between perceived and actual tilt. He states, "It will be shown that oscillation (or reversal of the actual rotation of the plane figure) occurs when *O*'s perception of the tilt of the plane does not coincide with the actual tilt" (3, p. 321). There are two possible interpretations of this assertion, (*a*) that perceived and actual tilt are disparate *only* during the reversal, or (*b*) that during the reversal perceived and actual tilt do not coincide. If he means the first, he is clearly wrong. It is true of some figures (a rectangle, for example), but the only time the trapezoid is seen in its true position is when it is end-on. Then there is no tilt to be seen, just a line. If he intends the second interpretation, the statement is tautological and has no explanatory value. *Of course* perceived and actual tilt do not coincide during the illusory reversal.

It seems fair to say, in summary, that although Pastore and Ames may be in basic disagreement, it is not yet clear wherein that disagreement lies. The supposedly contradictory evidence Pastore reports was secured under conditions not at all comparable to those which obtained in the experiment he criticizes, and a major share of his discussion is concerned with issues which a careful study of Ames's monograph shows to be nonexistent.

[1] Pastore no doubt means "rectangular" (the term used by Ames) rather than "rectilinear." A trapezoid is rectilinear (i.e., bounded by straight lines), but not rectangular.

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THE *PSYCHOLOGICAL INDEX*

The Psychological Index is a bibliography of psychological literature which was published annually for the American Psychological Association from 1895 to 1936. Thirteen of the forty-two volumes are out-of-print: numbers 2, 3, 4, 5, 9, 12, 17, 18, 19, 20, 22, 25, and 26. The remaining 30 issues are priced at $2.00 per volume, and sold in sets or separately.

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